THURSDAY, AUGUST 27, 1891.

THE CONGRESS OF HYGIENE.

WE continue this week our account of the work done at this Congress. It will be clear that with the space at our disposal it is only possible to refer to few among the many subjects discussed. Among these we have selected those which have the closest connection with those researches now attracting special attention.

In regard to the subject of tuberculosis it was certainly a happy inspiration of the officials of the Bacteriological (II.) and Comparative Pathological (III.) Sections of the International Congress of Hygiene and Demography, to call a joint meeting in order that a full discussion of the scientific and practical bearings of the questions relating to "the transmission of tuberculosis from animals to man by means of flesh and milk derived from tuberculous animals" might be possible; and it was also fortunate, as far as its success was concerned, that the discussion was opened by Profs. Burdon Sanderson and Bang, each of whom in his own sphere is singularly well fitted to lay before the members of the Sections what is at present known in the medical and veterinary scientific worlds concerning this important subject. Prof. Sanderson's early researches on tuberculosis have opened up the way for much of our present knowledge on the subject, in addition to which he has watched the question most carefully through its various stages of evolution; whilst Prof. Bang, by his numerous practical observations and scientific experiments, has given a completeness to our knowledge which has not been attained as the outcome of the work of any other observer.

The discussion on this question afforded another instance of the intimate connection between the purest research and the most practical affairs of every-day life.

Thus from the tenor of the discussion it may be gathered that the danger arising from the ingestion of tuberculous milk and meat has probably been exaggerated.

Some of those who took part in the discussion, for example, seemed to doubt whether primary tuberculosis of the alimentary canal—*i.e.* tuberculosis confined to this region and evidently the result of infection through the mucous membrane—was ever met with in adults, and even whether it was of very frequent occurrence in the child; whilst other speakers were able to instance out of their own experience certain cases of the former and many of the latter, strongly accentuating the fact that such primary disease of the intestinal canal does exist. Then, again, one speaker was convinced that Koch's bacillus had little or nothing to do with the production of tubercular disease; but the contention had been met by so many accurate observations and experiments that he may be said to have been ruled out of court, though it was on all hands agreed that the bacillus might be helped in its work by various predisposing causes, many of which were brought into full prominence during the discussion. It was also accepted that the tuberculosis of cattle is similar, as regards its causal agent, to the tuberculosis of the human subject, and that the disease is merely apparently modified owing to the different conditions, and perhaps delicate tissue modifications, offered by the different hosts of the parasitic bacillus; and from the most careful and detailed experiments, of which a large number were described, there seems to be no question that tuberculosis is communicable from animals to man, and certainly there appears to be none that it is communicable in the opposite direction.

There was a general expression of opinion as the outcome of the discussion that legislation of some kind or other is necessary, but, as pointed out by Burdon Sanderson, if laws were made to-morrow there is absolutely no staff of inpectors capable of giving effect to any that might be drafted. It is probable that this will draw attention, first, to the necessity for conferring powers of inspection of dairy and store cattle on some central authority; and second, to the necessity there is that our veterinary surgeons should undergo a thorough scientific and practical training, such as would fit them to fill the posts from which unfortunately they are necessarily now in many instances excluded.

When all is said and done, it appears that the danger arising from the consumption of tuberculous meat is far less serious than that involved in the consumption of milk from tuberculous animals, as meat, if *thoroughly* cooked, appears to be perfectly innocuous, the tubercle bacilli being readily destroyed by heat, whilst the nutrient properties of the meat itself are little, if at all, interfered with by judicious cooking. In the case of milk, however, in which the presence of tubercle bacilli has been so often demonstrated, it has to be borne in mind that boiling so alters the constituents of the milk, especially the proteids, that it is rendered very much less digestible; and its nutritive value is greatly interfered with.

We now pass to the discussion.

TUBERCULOSIS IN ALL ITS RELATIONS.

Prof. Burdon Sanderson said the subject which he had undertaken to bring before the notice of the conjoint Sections for discussion was one of the gravest importance, for there was no disease, acute or chronic, which was so productive of human suffering or so destructive of human life. In a Congress of Hygiene the subject of tuberculosis could only be considered in relation to its causes, the aim of hygiene being to prevent disease, not to cure it. He wished specially to direct attention to those questions which relate to the dangers which are alleged to arise from the use of tuberculous food. (1) Does general tuberculosis in man originate from intestinal infection? (2) If it does, is it possible to guard against so fearful a danger? For the purpose of avoiding useless discussion on subjects on which there ought to be perfect agreement of opinion, he asked that certain fundamental propositions should be accepted as settled; such as, for example, the existence of a *materies morbi* in the form of the tubercle bacillus, its constant association with the tuberculous process, and the identity of human with bovine tubercle; and also that it be assumed that any part of the body of a tuberculous animal or any secretion of such an animal would, if it contained tubercle bacilli, be a source of danger, and that the use of such liquid or part ought to be prohibited or avoided. This being understood, we were in a position to enter on the questions which require answers, some of which are pathological or etiological, the others practical or administrative. The etiological questions might be said to relate to the three possible ways in which a human being may be infected by tubercle—namely, inheritance, pulmonary inhalation (atmospheric infection), and food (enteric infection). The practical issues were—

(1) Is the risk to the individual consumer of such a nature that it can be detected and estimated?

(2) Is it of such a nature that it can be counteracted?

(3) Is the collective risk to which the community is exposed sufficient to demand the interference of the State? and

(4) If it is, How can the State interfere with effect?

Of the two practical questions which relate respectively to infection by milk and to infection by meat, the latter was very largely discussed at a Congress on the subject of tuberculosis held in Paris in 1888, and has again been discussed very recently. In the first of these debates the medical profession did not take a very prominent part. The question whether the flesh of tuberculous animals is dangerous or not was regarded chiefly from the point of view of the veterinarian.

In 1888, M. Arloing, following out the principles enunciated by another gifted pathologist, the late M. Toussaint, that tubercle is a disease *totius substantiæ corporis*, maintained that the time had come to act "conformant à la logique." One out of every six carcasses had been shown, he said, to be infective, when tested by administering it to test animals as food. He calculated that over one thousand persons joined in the consumption of every such carcass, and consequently that one-sixth of this number—that is, about 170 persons—must be subjected to the risk of infection for every animal sent to the shambles. If this reasoning were true, if we could measure the danger to the human consumer by the presence of tuberculosis among animals used for food irrespectively of other considerations, then M. Arloing was right in his practical deduction from it that whatever interests conflict with public health they must give way. It was our duty to insist on the right of science to dictate; but in doing so it was necessary to be careful not to do so until the question had been looked at from all sides and the whole evidence had been heard.

In some of these discussions it had not been sufficiently considered that the question was not whether the consumption of tuberculous meat was in itself attended with risk, but whether the presence of tuberculous diseases among ourselves was in any way due to the fact that we occasionally eat meat which contained bacilli. It was not sufficient to show that on the one hand there was a fearful mortality from tuberculous diseases, and that on the other there existed a cause to which this calamity might be attributed. It must also be shown that the effect was actually produced by the cause, in such sense that if the cause were removed we might hope that the effect would disappear.

Twenty-three years ago Chauveau fed three heifers with tuberculous material from the body of a cow and obtained positive results. At that time the idea that tuberculosis was a virulent disease was new. M. Villemin had made his great discovery, but it had not yet been accepted, and consequently Chauveau's results were severely criticized, and were the subject of much discussion, which extended over several years (1868–74), during which he repeated his observations, effectually silenced his opponents, and determined with the greatest exactitude all the conditions which are required to insure success in the experimental production of tuberculosis by feeding. Gerlach about the same time made similar experiments in Germany which led him to advocate in the most energetic manner the restriction of the sale of tuberculous meat.

These two initial investigations were followed by many others. In 1884, Baumgarten showed that a couple of ounces of milk to which a pure culture of tubercle bacillus had been added were sufficient to produce characteristic tuberculosis in the intestines of a rabbit; and that the effect of such feeding was so constant that by examining the animals so fed at successive periods all the stages of the process could be thoroughly investigated, the most important result being that after a period of latency of a fortnight, during which no traces of infection were visible, the lymphatic follicles of the mucous membrane and the mesenteric glands began to enlarge simultaneously without any change whatever in the intestinal epithelium.

It was thus shown with a precision which was not before obtainable that the initial phenomenon of tuberculosis was primarily a proliferation of the adenoid tissue of the lymphatic system, and that the bacillus was capable of finding its way into the lymphatic system without leaving behind it any appreciable traces of its presence at the portals by which it had gained admission. Since 1884 our knowledge of the subject had been still further advanced by Cornil, under whose direction two very important researches, confirming and extending Baumgarten's results, have been recently published, from which it was evident that when the tubercle bacillus is absorbed from the intestine it follows the course of the lacteals, and that the lesions which it produces correspond closely with those which present themselves in those rare instances in which it is possible to observe the first beginnings of enteric tubercle in the human subject.

Much, however, has still to be learned by the experimental method—information which could only be gained by observations on animals. According to those who regard tuberculosis as necessarily a disease, *totius substantiæ corporis*, in which every part of the body is contaminated, all meat derived from the body of a tuberculous animal ought to be condemned, whether it appears healthy or not, for they argue that in every such animal, however localized the disease may be, bacilli circulate in the blood, and are so universally distributed.

Prof. Sanderson believed that this was not true, and that we are not entitled to assume that the flesh of every tuberculous animal is infectious unless it be proved to be so. As against the probability of its being so, it must be noted that the tuberculosis of cattle, although the product of the same bacillus as the tuberculosis of man, is a disease of comparatively slow progress. It localizes itself in structures which are not essential to life, and nutrition might be so little interfered with that the animal could be readily fattened for the market. There was no doubt that the flesh of such animals might be to all appearances in good condition, and might be offered for sale as meat of prime quality, and as yet we have no evidence that it is infective.

Turning from the source of infection to its effects, from the bacillus to its field of disease and death-producing action, Prof. Sanderson said that tuberculous diseases contribute something like 14 per cent. to the total of deaths from all causes, and that during childhood, as distinguished from adult life on the one hand and from infancy on the other, tuberculous mortality scarcely amounts to a quarter of this percentage, whereas in infancy it only falls a little short of it, and in early adult life, it very far exceeds it.

There was evidence that under certain conditions the virus of tubercle was absorbed by the lymphatic system from the small intestine in man, and that when this happens it may give rise to lesions of the same nature as those produced in animals by the injection of liquids in which bacilli are suspended—that is, to lesions which originate in the lymphatic system. Tuberculous disease of the intestinal mucous membrane, although very common, never occurred in the adult and very rarely in infancy as a primary disease. In the adult it might occur as an ulterior consequence of pulmonary consumption, the way in which it occurred being very evident. In the advanced stages of that disease muco-purulent liquid was discharged in quantity from the softened parts. This material charged with virulent bacilli might infect the mucous membrane along which it passed so that it is easy to distinguish bronchi which lead from vomicæ by the tuberculous nodules with which they are more or less beset. In advanced phthisis the sputum is so abundant that a certain proportion of it is from time to time swallowed. No effect is produced in the œsophagus or stomach, for along the former it passes too rapidly, while in the latter the mucous membrane is effectually protected by the gastric juice, which, although incapable of devitalizing the bacillus of tubercle, arrests its development. In the alkaline contents of the small intestine a condition more favourable to its development was found, and from there it was absorbed, just as any other particle of similar size might be, by the lymphatic follicles. Tuberculous disease of the small intestine in the adult thus occurred. It was always a secondary result of pulmonary phthsis.

In childhood the case is different. Tuberculosis does not begin to assert itself as a cause of death until the third month of extra uterine life, but after this there was good reason for supposing that the bacillus plays an important part as a cause of mortality.

To the pathologist the question of how latent tuberculosis of the lymphatic system or of bone originates, *i.e.* how the bacilli which produce them are introduced into the blood stream was one of great interest. Prof. Sanderson confessed it to be his belief that in a certain proportion of cases the cryptogenetic tuberculoses were due to causes which operate before birth. From Dr. Muller's Munich statistics it might be gathered that in less than half of the cases in which the lymphatic glands are found to be tuberculous the affection has its seat in the mesentery, and that the mucous membrane of the intestine is tuberculous in a still smaller proportion—less than a quarter. In many of these cases the mucous membrane was no doubt affected subse-

quently on tuberculous disease of the lungs, but in the remainder the disease seemed to be primary. If it could be proved that such cases were primary, the fact would afford clearer evidence than any we now possess of the enteric origin of tuberculosis.

In the absence of such proof, human pathology had very little indeed to say in favour of the belief that human tuberculosis could owe its origin to the consumption of tuberculous food, and even if it were proved that the absorbents afforded a channel of entry for the tuberculous virus in children it would have little significance as regards the consumption of meat.

The author held, therefore, that we are not as yet in a position to demand the interference of the State on the ground that the community actually suffers from the consumption of tuberculous meat, the evidence that it is so being too weak to be insisted on; but he maintained that the consumption of tuberculous meat was attended with some danger, and that on that ground its consumption ought to be prevented by the State and avoided by the individual.

As regards the administrative question, he held that if we had, to-morrow, a law forbidding the sale of any meat containing the bacillus of tubercle, it could not be carried out unless those charged with its administration were able to distinguish such contaminated meat from healthy meat, so that the efficiency of the law would depend on the question whether the art of discriminating between infecting and non-infecting meat had attained to such perfection as to enable an adequately trained inspector to exercise his function with effect. The practical result to which we have come was this. Everything must turn on diagnosis. The Legislature might direct that all meat intended for consumption should be subjected to inspection, might appoint inspectors, impose penalties, and provide just and adequate compensation, but all this would be of no use unless the principles on which the discrimination of infecting from non-infecting meat is to be founded could be laid down, and the services of skilled persons of sufficient intelligence to apply them could be secured. We might consider it quite certain that in this country at least it would at present be extremely difficult to find such persons. Not that the veterinarian was less capable than the doctor of making a scientific investigation, but that he does not possess, and has, as yet, had no opportunity of acquiring, the sort of skill which is necessary for making what the French call the *diagnose précoce* of tuberculosis. Two things in short are required, neither of which we have at our disposal—special scientific knowledge and technical skill, and the former of these must be acquired first. Science must determine, much more definitely than has been done as yet, what are the earliest changes which have their seat in the parts of animals used for food, and which of these might indicate danger to the consumer. This knowledge could only be acquired by experiments specially made for the purpose, and having been attained it could only be applied by technically trained persons. He illustrated the sort of skill required by comparing it to that possessed by the professional tea taster as regards the commercial value of tea. Why was the judgment of the expert reliable? Because he was responsible for it and was paid for it. It would be the same as regards the early recognition of tubercle in cattle, if skill and discrimination were paid for; and the same moment that this skill was required it would come into existence. What would be wanted in the inspector was not that he should be a pathologist or even a bacteriologist, but a trained expert; for although the rules unconsciously used by him might be based on scientific principles, it is not by these principles he is guided in each case, but by practical skill.

Dr. Sanderson then submitted the following propositions to the meeting of the combined Sections:—

(1) That tuberculosis must be added to the list of diseases regarded by the law as contagious. There is no sufficient reason for supposing that in the human adult the introduction of the bacilli of tubercle by enteric absorption is the efficient cause of tuberculosis. In infancy a large proportion of the apparently idiopathic tuberculous diseases of the lymphatic system are probably due to the penetration of bacilli into the organism from the intestine; but the evidence which we at present possess on this subject is not sufficiently precise or extended to serve as a basis for prophylactic action. For this reason the origin of tuberculosis in infancy is a subject which urgently requires investigation.

(2) It has been proved that the ingestion of any material which contains the bacilli of tubercle is a source of risk to the consumer, but the conditions which limit this risk are insufficiently known. It would, therefore, be unjust to enforce the destruction of any specimen of meat apparently healthy, even though it were known to be derived from a tuberculous animal, excepting on evidence given as regards the particular case that it would be infecting if administered to test animals.

(3) As regards the duty of the State in relation to the prevention of tuberculosis, what is immediately required is that an efficient system of skilled inspection should be created. This is desirable, not merely as a first step towards a prevention of the sale and consumption of tuberculous meat, but as an indispensable means of acquiring better information than now exists. To be of use it must be carried out on the principles I have already set forth. It must be conducted by men of technical skill acting under scientific guidance.

"In conclusion," said Dr. Sanderson, "I would beg you to notice that I have limited myself to the question of the consumption of meat. Although I have purposely left the milk question out of consideration, I have referred to facts which bear upon it. We have seen it to be exceedingly probable that about 40 per cent. of the children that die in hospital, die tuberculous. I have already expressed my belief that in some of these cases the disease is congenital—that is, dependent on causes which have operated before birth. Some are probably infected by inhalation of the tubercle bacillus from the atmosphere, notwithstanding that pathology affords so little evidence of it; but for the rest, notwithstanding the lack of satisfactory evidence, I cannot resist the conviction that the consumption of unboiled milk during the years which follow weaning must have its share in bringing about the fatal prevalence of tuberculous disease at that period of life. This being the case, I feel that, whatever course may be taken as regards meat, I can join heartily with those who think that the sale of contaminated milk ought to be put a stop to by all possible means, and I trust that on this subject there will be no difference of opinion, and that this Congress will take such action as may promote the progress of legislation."

Dr. Bang, Lecturer in the Royal Veterinary College, Copenhagen, in a paper on "The Alleged Danger of consuming the apparently Healthy Meat and Milk of Tuberculous Animals," stated that the great majority of investigators are agreed that the essential source of tuberculosis in man is found in man himself; but almost all admit that he may contract the disease through the ingestion of milk derived from animals affected with tuberculosis.

It is always agreed that such a danger exists, but as to the extent of the danger there is little unanimity.

Of course, it might be said that there would be no danger if the use of meat and milk from the tuberculous animals were entirely interdicted; but it must not be ignored that the application of such a stringent measure would entail enormous loss from an economical point of view, especially in those countries where the disease has a very wide distribution amongst bovine animals. He looked upon the general application of the French regulations as out of the question, at least for the present, whilst such a course appeared on the whole to be unnecessary. As regards milk, the question of prophylaxis was comparatively easily settled if it was resolved that it should never be employed without first being boiled. But then the question comes to be, How can we protect ourselves against the products of milk?

The experiments made by Galtier, the author, Hum, and others have proved that the various products derived from milk, butter, cream cheese, cheese, and butter-milk may all contain tubercle bacilli, and that these retain their vitality in such products for a period of from fourteen to thirty days. It was true the majority of these bacilli may be separated from milk if the cream be removed by means of a centrifugal machine, as is generally done in Denmark, but if the milk is very rich in bacilli a few usually remain in the milk, and even in the cream. In order to do away with this danger it is necessary to expose the milk or the cream before churning to a temperature high enough to kill the tubercle bacilli (85° C. for about five minutes); a temperature of from 60° to 75° C., however, being quite sufficient to attenuate the organic virus, so far as to render it incapable of setting up infection of the alimentary canal. This method is coming more and more into use in Denmark, as by it several other sources of infection in the butter are also neutralized. As, however, many people object to the taste of boiled milk, it became an important question to determine whether the milk of phthisical cows is really a source of danger in the majority of cases. He had determined

that when the udder is affected with tuberculosis there are usually numerous bacilli in the milk, which is consequently extremely dangerous. But he also finds that mammary tuberculosis is not so common as was at one time supposed. At the *abattoir* of Copenhagen, for example, it has been found that only in 1 per cent. of tuberculous cattle was there disease of the udder. From twenty-eight tuberculous cows, in which, however, there was no disease of the udder, the milk was injected into forty-eight rabbits, and in only two was there any positive result. He then inoculated forty guinea-pigs with milk from twenty-one tuberculous cows, in this case with four positive results. Recently he had carried on a new series of experiments with the milk from fourteen extremely phthisical cows. In this series the milk was virulent in three cases, so that from sixty-three tuberculous cows the milk contained virulent tubercle bacilli in nine cases only. All these cows were affected in a very high degree, and it is probable that in some at any rate the udder was affected; though this could not be demonstrated in the living animal, as it was in three out of the four cases of the second series. Others were affected with miliary tuberculosis in the different organs, a condition which one rarely finds in an animal that is still giving milk, and in one case the supramammary lymphatic glands were affected with tuberculosis, although no lesions in the udder itself could be demonstrated.

In several of the positive cases the number of bacilli in the milk must have been very small, as one only of the two guinea-pigs experimented upon succumbed to the disease, this happening in three instances.

It should be added that the quantity of milk injected in the later series was larger than in the earlier series. In the two first series 1 to 3 c.c. was injected, in the third 5 to 10 c.c. He maintained that, although in many cases the milk from phthisical cows is not virulent when the mammary gland is unaffected, it is in a certain proportion of cases, and should always be looked upon with suspicion, and that it is absolutely necessary to take prophylactic measures against the use of such milk, although the danger should no doubt not be exaggerated.

Meat.—Flesh itself very seldom contains any tubercle. Nevertheless it had been proved by a number of experiments that the muscle juice may contain tubercle bacilli, but such cases, according to the observations of Chauveau, of Arloing, Peuch, Galtier, Nocard, Kastner, and others, are absolutely in a minority. Amongst seventy-three phthisical cows these observers have found only ten in which the muscle juice gave evidence of virulence on injection into rabbits or guinea-pigs, and sometimes the juice inoculated only produced the disease in one of several animals inoculated.

M. Nocard's experiments in this connection are very interesting. He found that when a culture very rich in bacilli was injected into the vein of the ear of a rabbit, the muscle juice of the animal was virulent only when it was killed within five days after the inoculation, from which he argued that the bacilli carried by the vessels to the muscles only preserve their vitality for five days. If to this experimental result be added the observation that tubercle is very seldom developed in the muscles, even during the development of a condition of general tuberculosis, it must be concluded that muscular tissue is a soil so unfavourable for the growth of tubercle bacilli that they are not able to multiply. The number of bacilli, then, that can be found in the flesh of tuberculous animals is always extremely limited. It is of course true, as M. Arloing has objected to M. Nocard's conclusions, that the circulatory system of a tuberculous animal can continually receive into it fresh bacilli, and therefore until within only a few minutes before the animal is slaughtered. But, on the other hand, it must not be forgotten that it is only in the case of the development of an acute miliary tuberculosis that one can suppose that the number of bacilli introduced into the vessels can be considerable. In ordinary cases in which the tubercular process is developed slowly the bacilli would without doubt escape into the blood in very small quantities, and the number of bacilli that could be found at any given moment in the meat would be very small. Moreover, the experiments carried out by Galtier, Gebhardt, and others, render it very probable that the number of bacilli introduced into the alimentary canal, by which infection does not readily occur, plays a not unimportant part in the result obtained.

Prof. Bang stated that he had recently completed a series of experiments on the virulence of the blood of cows in an advanced stage of tuberculosis. From twenty tuberculous cows he inoculated thirty-eight rabbits and two guinea-pigs with defibrinated blood, injecting from 10 to 18 c.c. (in four cases only 5 to 9 c.c.). In eighteen cases the results were negative, in two positive, and one of these in which the lesion was small was one of two rabbits injected with blood from the same cow. The cow that supplied the blood with which the other positive result was obtained had developed acute miliary tuberculosis after an injection of tuberculin. Three weeks previously blood from the same cow had given negative results. Even amongst those cases in which the results were negative there were several cases of acute miliary tuberculosis.

He concluded from the foregoing that the seizure of all tuberculous animals is too stringent a measure. So long as the tuberculosis is strictly localized, the meat is not a source of danger; where the malady is generalized, the consumption of the meat may be dangerous, although it is not always so. The eating of uncooked meat should be discouraged, but the best means of avoiding danger to the health of man is to take all possible measures for preventing the propagation of tuberculosis amongst our domestic animals.

Prof. Arloing, of Lyons, contended that the question of transmissibility of tuberculosis from animals to man was one of very great importance, but he admitted that the *diagnose précoce* was very difficult. The danger to children of drinking milk from tuberculous cows was great, and he thought could scarcely be exaggerated. Moreover, he held very strongly that, except under certain special circumstances, the total condemnation of tuberculous meat was necessary, and on grounds of public health he dissented entirely from Dr. Bang's position.

The flesh of all tuberculous animals should be suspected as dangerous to health, the more so as meat was very often insufficiently cooked, the bacilli present under these conditions remaining pathogenic. From statistics he had gathered, he felt no doubt on this subject, and although it might be possible, by first carefully cooking under public supervision, to allow the flesh from animals in which the tuberculosis was localized to be sold, he still maintained his position that total confiscation of tuberculous meat was the safest method to be adopted. It was necessary, however, that in the first instance we should have a system of strict inspection, not only in our large towns, but also in all the smaller centres of population.

A paper was then given by Prof. M'Fadyean (Edinburgh) and Dr. Woodhead (London), on the transmission of tuberculosis from animals to man, by means of flesh and milk derived from tuberculous animals. They maintained that the evidence as to the transmission through the flesh or milk of tuberculous animals was very conflicting, apparently in great part because the methods used were different, and the conditions were not uniform. They had attempted to follow the line of infection of tuberculosis in a number of children, and had found that in 127 cases analyzed tubercle of the intestine was present in 43; 24 of these cases occurring between one and five and a half years; tubercle of the mesenteric glands was found in 100 cases, or in nearly 79 per cent. of the whole; here, again, 62 of these occurring between one and five and a half years; and of 14 cases in which the mesenteric glands were primarily affected—*i.e.* no trace of tubercle could be found in any other part of the body—9 were referred to the same period. It was noticeable that of these 100 cases only 20 were diagnosed during life as suffering from abdominal tubercle. From all that could be learned from these cases (and reference could be made to a large number of other sets of statistics practically proving the same point), it was evident that intestinal and mesenteric tubercle are most frequently met with in children during the period after they are weaned, at which time cow's milk has been substituted for mother's milk. The point of entrance appeared in these cases to be by the intestine. They had come to the conclusion that in some cases at least the tubercle bacilli had passed from the intestine into the mesenteric glands without leaving any trace of lesion to indicate their point of entrance. There could now be no doubt that tubercle bacilli were sometimes present in the milk from tuberculous cattle, especially where the udder was affected, and they had been able to obtain such bacilli embedded in the epithelium of the milk ducts, or lying free in the ducts after the death of the animal. They concluded that wherever the presence of a tuberculous condition of the udder could be demonstrated clinically it would be little less than criminal to give the milk to delicate children, or even to children suffering from any catarrhal derangement of the intestine, a condition that is specially frequent amongst the

poor classes, where the standard of health is exceedingly low and the liability to catarrhal conditions very great. From a series of inoculations with tuberculous udder, and with milk from tuberculous udders, 14 out of 19, or over 70 per cent., had given positive results; with non-tubercular udders, and with milk from otherwise tuberculous cows, only 2 cases out of 13, or a little under 16 per cent., gave positive results. Where the failure to produce tuberculosis occurred in the first series, the number of bacilli was invariably small, and inoculations were usually into the subcutaneous tissue, though negative results were also obtained when other methods of infection were employed. They thought that in relation to the danger of taking tuberculous milk by the human subject, the site of the infection, and the relation of the number of bacilli introduced, played an important part in determining the severity and rapidity of the course of the disease, and they stated that their experience accorded with that of other observers, that inoculation into the peritoneal cavity is much more certain than inoculation into the subcutaneous tissue, especially where the number of bacilli introduced is comparatively small. They are also led to believe, from a number of feeding experiments, that the production of tuberculosis through the introduction of bacilli into the alimentary canal is of still less frequent occurrence than when inoculation is made into the connective tissue. As regards the possibility of the flesh of tuberculous animals setting up tuberculosis, (*a*) when introduced *en masse*, (*b*) when expressed juice only was exhibited, their experiments went to prove that the juice only did not in most cases contain a sufficient number of bacilli to set up tubercle, even when inoculated into small rodents, but from the fact that they have observed tubercular masses in the muscles of the buttock of tuberculous cattle, it must be accepted that tubercle bacilli may sometimes, though perhaps rarely, be present in considerable numbers in this position. Of three cows slaughtered in one day at one slaughter-house, well-defined tubercle was found in the muscles of the buttock of two animals; in one of these there was tuberculosis in every organ and part of the body; in the other there were only a few nodules and in some of the glands; there was certainly no pleural or peritoneal tubercle, and all the other organs were unaffected. They concluded that there was great necessity for a thorough inspection of both dairy cattle and of animals that were slaughtered for food purposes, but it might be accepted that the danger of contracting tubercle from milk was greater than that of contracting it from meat, and that only in a certain proportion of cattle affected with tuberculosis did there seem to be any danger to be anticipated from the ingestion of the flesh. In the main they agreed with Prof. Burdon Sanderson and Dr. Bang that there was not yet sufficient evidence on which to decide that the total seizure of meat from tuberculous animals should be resorted to.

Prof. Hamilton, of Aberdeen, said that there were two principal channels of infection, (1) the gastro-intestinal tract, (2) the lungs; but in addition to these we had what might be spoken of as localized tubercle, which seemed to be shut off entirely from all communication with the external world. (1) In the body the affection might take place by the air channels, as in the case of tubercular pneumonia, where the virus was probably inhaled and the air vesicles were the primary seat of infection. (2) By the blood vessels, as in cases of eruption of miliary tuberculosis. (3) By the lymphatic vessels, as in the more chronic forms of tuberculosis. In the gastro-intestinal canal a tubercular lesion might accompany an ordinary phthisis; it was often seen in children as a primary condition, and he should not be inclined to agree with Dr. Burdon Sanderson that it was not also primary in adults, as he himself had seen several cases, one quite recently. Previous catarrh was not always met with in the lung, but it was certainly a predisposing cause of tubercle, as it interfered with the protective epithelial covering. When tubercle followed whooping-cough, measles, and so on, it was probably the result of the spread of infection from pre-existing caseous spots, or it might be that the glands, weakened by the disease, fell an easy prey to the tubercle bacillus. He could not understand the comparative immunity from tubercle enjoyed by the pericardium and the stomach.

Prof. Nocard, of Paris, did not think that sufficient proof had as yet been accumulated that ingestion of tuberculous meat could give rise to tuberculosis in any large proportion of cases; the greater number of experimental cases had given negative results, and he should, to convince himself, require to see more positive results obtained in which all possible sources of failure could be eliminated. Whilst saying this, he must admit that in the case of children tuberculous material, whether in meat or milk, would always prove a very important source of danger. He would draw attention to the disease as it occurred in cats, on which animals he had made many experiments.

Dr. Hime, of Bradford, was glad to find that our foreign friends, who are not hampered as we are in making experiments, agree with us in the main. He thought that we were likely to run wild on the subject of the total seizure of tubercular meat, and he would point out that in no country does a total seizure law exist such as it is proposed to adopt here in England. In England he would point out that the inspection is worse than in any other country. He referred to Prof. Lingard's experiments given in an official report, which, he pointed out, spoke only of tubercle being transmitted by caseous material, and not by meat from a tuberculous cow, as was usually assumed. We had the authority of Koch himself, said Dr. Hime, that there is danger only when tubercular material itself is ingested. Infection by milk he looked upon as proved, but he would also insist very strongly that the majority of infection in cases of phthisis was directly between man and man, and it was far more important that we should eliminate possible sources of contagion between human subjects than that we should pay so much attention to the minor possibilities of infection from animals to man.

Dr. Barlow (London), speaking from a clinical point of view, was scarcely able to indorse the results of experimental researches, and he maintained that as regards tuberculosis in children we must for the present keep our minds open. There was no doubt that the *post-mortems* in children's hospitals gave evidence of the enormous frequency of tuberculosis, but the evidence that such disease was the result of the ingestion of milk and meat was comparatively slight. Other sanitary precautions, which he looked upon as of primary importance, must not be lost sight of in our discussion of the subject. He would, however, enter a protest against the use of the raw meat juice in the case of delicate children, as from what we had heard it was evident that such aliment might prove a source of considerable danger.

Prof. Perroncito, of Turin, referred to a number of experiments that he had carried out with meat, milk, and the products of the latter, and then pointed out that spontaneous tubercle very rarely occurred in the pig, though it might frequently be met with as the result of infection. The same might be said of sheep. Here, also, it might occur, though rarely, as the result of direct infection.

Prof. Burdon Sanderson, in reply, said he was pleased to find that the difference of opinion amongst so many authorities was so slight. It was evident that all were agreed that inspection was necessary, and there was also a general consensus of opinion as regards the difficulty of diagnosis. He was glad to find that although M. Arloing still retained his opinion as to the necessity for total seizure, except under very well-defined conditions, he had so far given way as to acknowledge that such meat might after careful cooking be retailed under special restrictions. In order that something definite might come out of this discussion, he proposed that it be minuted that "the etiology of tubercular disease of early infancy (between three months and five years)" be referred for discussion at the next Congress.

This was seconded by Dr. Septimus Gibbon, and was carried unanimously.

The President said that he had been greatly interested in the discussion, and he hoped that much good should arise therefrom. He was glad to find that there were some animals, such as the sheep and pig, in which spontaneous tubercle was never met with, and he hoped that we might eat these in safety. Sheep especially appeared to have a great immunity as regards tubercle, but pigs were not so safe, as they were apparently frequently the subject of tuberculosis.

Dr. Metschnikoff and Dr. Roux gave a joint paper on the changes that took place in the tissues around tubercle bacilli. It was read by the former, who illustrated his remarks by means of drawings on the black-board, and by microscopic specimens. They indicated the difference in the reaction of our tissues to the tubercle bacilli when the disease is going to run a favourable course, and when the animal is about to succumb rapidly to the disease. The process of recovery was indicated by the presence of concentric rings of hard and inflammatory tissue around the bacilli, which eventually lead to their absorption, the inflammatory tissue itself finally undergoing a process of calcification.

Prof. Ehrlich proceeded to give Koch's present views regarding tuberculin. He said that the results that had been obtained were exceedingly favourable, and most of those who had failed to obtain equally good results had failed because they had used too large doses of the remedy. The principle of cure rested in the local effects which tuberculin exercises on the specifically affected tissues; the inflammatory reaction passing to necrosis was neither desirable nor necessary, but, on the other hand, slight and even repeated stimuli would so act as to give rise to cicatrization of the tuberculous centres, so that the essence of this method of treatment was to retain as long as possible the specific excitation of the tissues, and not to do away with this, as was the case where large doses were used. Wherever successful results had been obtained they had all been by the use of repeated minute doses of tuberculin, which were only very gradually increased in strength, and it should be specially noted that the pathological signs found as the result of the action of tuberculin were always produced by large doses.

Prof. Cornil, Dr. Bardach, Dr. Ponfinck, and Prof. Hueppe were agreed that tuberculin was an heroic and dangerous remedy about which we as yet knew little, and which was therefore to be looked upon as still being experimented with. It also seemed to be the general opinion that where it was in use there existed a danger of setting up generalization of a tuberculosis that had hitherto been localized.

Dr. Hunter gave the results of his own experiments (described in the *British Medical Journal*), from which he had been able to show the nature of the active principle of tuberculin. He had succeeded in isolating principles quite different from those mentioned by Koch, or even reported by Dr. Ehrlich that morning as having been obtained by Koch. They were three—(1) those which produced fever, but set up no local reaction; (2) those which gave a local reaction, but no fever; and (3) those which set up neither fever nor local reaction, which had a distinctly remedial effect.

The President, summing up, hoped that in time we should all be able to obtain the wonderfully satisfactory results that had been so fully described by Prof. Ehrlich on Dr. Koch's behalf.

LETTERS TO THE EDITOR.

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Rain-gauges.

I DO not think that valuable space in your columns should be occupied by rediscussing old questions. I do not wish to say a word in any respect discourteous to Mr. Fletcher, whose ability in other subjects has, I understand, been already recognized, but it really would have been better had he read up the subject before writing the remarkable letter which appears in NATURE of the 20th inst. (p. 371).

For experimental work, spherical, conical, inclined, horizontal, vertical, and tipping funnels have been used; but until the soil of the British Isles can be made to tilt in altitude and rotate in azimuth, so as to meet the path of falling rain, I think that we must adhere to gauges with horizontal mouths as the best representatives of the surface of the earth.

G. J. SYMONS.

British Association Reception Room, Cardiff, August 21.

Cloud Heights—Kinematic Method.

IN NATURE of April 16 (p. 563), and possibly elsewhere, I am made to speak of the method of determining the heights of clouds at sea used by Finemann and myself as the "aberration method." This was a misnomer that I supposed had been corrected. The more proper term is the "kinematic method," since in it we discuss the apparent motions of the clouds considered as the resultant of the true motions of the cloud and the observer. This is the term that I have used since May 1890, and would commend to others.

CLEVELAND ABBE.

Weather Bureau, Department of Agriculture,
Washington, August 8.

THE BRITISH ASSOCIATION.

THE Cardiff meeting, if it was not made remarkable by any incident of very special importance, was, upon the whole, successful. Several of the addresses delivered by the Presidents of Sections were of exceptional interest, but some were very long, and we shall not be able to print all of them.

At the first meeting of the General Committee, held on Wednesday, August 19, the report of the Council for 1890–91 was read by Sir Douglas Galton. Dr. Gladstone moved a vote of thanks to Prof. Williamson for his long and valuable services as general treasurer, paying a tribute to the manner in which that gentleman had fulfilled his duties. Sir Douglas Galton seconded, and the resolution was cordially agreed to. Mr. Vernon Harcourt moved, and Sir J. Douglass seconded, the appointment of Prof. Arthur Rücker as general treasurer. This motion was also agreed to. At the meeting of the General Committee on Monday, a deputation from Nottingham was introduced. The Association was invited by the Mayor and town authorities to visit Nottingham in 1893. It was stated that it was twenty-five years since the Association had visited Nottingham. The invitation was accepted on the motion of Mr. Preece. It was also unanimously agreed, on the motion of Canon Tristram, to elect Sir A. Geikie as President of the Association, which meets at Edinburgh next year. The Lord Provost of Edinburgh, the Marquis of Lothian, the Earl of Rosebery, Lord Kingsburgh, Principal Sir William Muir, Prof. Sir Douglas Maclagan, Sir William Turner, Prof. Tait, and Prof. Crum Brown were elected Vice-Presidents for the Edinburgh meeting. Prof. G. F. Armstrong, Principal F. Grant Ogilvie, and Mr. John Harrison were elected Local Secretaries for the meeting at Edinburgh, and Mr. A. Gillies Smith Local Treasurer. A deputation from Edinburgh also attended with reference to the fixing of a date for the Edinburgh meeting. It was stated on behalf of the Town Council that September 28 was favoured as the opening date of the meeting, though August 3 and September 21 were also mentioned as alternative dates. A motion was made to fix August 3, while an amendment was moved for September 12; but as only thirteen voted for the amendment, the original motion was agreed to—that is, the Association will meet at Edinburgh next year on August 3. The general officers were re-elected, and the following gentlemen were elected Members of Council for the ensuing year:—Dr. W. Anderson, Prof. Ayrton, Sir B. Baker, Mr. H. W. Bates, Prof. Darwin, Sir J. N. Douglass, Prof. Edgeworth, Dr. J. Evans, Prof. Fitzgerald, Sir Archibald Geikie, Mr. R. T. Glazebrook, Profs. J. W. Judd, Liveing, Lodge, Mr. W. H. Preece, Profs. W. Ramsay, Reinold, Roberts-Austen, Schäfer, Schuster, Sidgwick, Mr. G. J. Symons, Profs. T. E. Thorpe, Marshall Ward, Mr. W. Whitaker, Dr. H. Woodward. The following impressions have been recorded by a correspondent:—

CARDIFF, *Tuesday Evening.*

One of the most prominent features of the Cardiff meeting has undoubtedly been the prevailing bad weather. Rain and cold have had their usual depressing results, and may to some extent account for the disappointment which exists among many of those in attendance. The Local Committee have done their best to render the meeting a social success, but the entertainments by the Municipality and the citizens of Cardiff have been of a somewhat restricted character. Notwithstanding the unpromising state of the weather, the excursions on Saturday and Sunday were largely taken advantage of, and the reception given by Lord Windsor on the latter day was specially appreciated. The total attendance has been about 1500, within 200 of the Leeds meeting, while the amount of money available for grants is within a few pounds of last year. Naturally there has

been considerable talk with reference to the address of the President of Section A, and opinion is divided as to the propriety of introducing the metaphysical into a Section which has emphatically to do with the "solid ground of Nature." On the other hand, Prof. Lodge's experiment to test whether the ether is disturbed in the presence of a rapidly-moving body has excited the greatest interest and admiration.

The *soirées* at the present meeting can hardly be compared in attractiveness and brilliancy with those held last year in Leeds. Wealthy and populous as Cardiff is, she has not command, apparently, of the scientific and artistic collections which are so creditable to the intelligence and taste of the dingy Yorkshire city. However, the dance into which to-night's *conversazione* developed evidently atoned for a multitude of shortcomings. The lectures have been fairly well attended, Prof. Rücker's beautiful experiments evidently fascinating his audience, in spite of a serious hitch caused by the failure of a steam-engine to do its duty when called upon. The discussion, in Section D, as to the relations between animal and plant life was well sustained, and it is a pity that arrangements had not been made to have it fully reported. This can be done at very small cost, and the publication of detailed reports of such discussions could not but greatly increase the good they are calculated to do. There is a general belief that inter-Sectional discussions would be of immense advantage in showing the intimate relations which exist between the different branches of science, and in stimulating research in profitable directions. It is probable that several such discussions may be arranged for the next meeting.

As usual, Section E had its sensation. A very large audience attended to hear Mrs. French Sheldon describe her journey to Lake Chala, at the base of Kilimanjaro. Mrs. Sheldon was evidently suffering greatly from her serious accident; and although her address was somewhat disjointed, it contained a good deal of fresh information, especially on the natives, which male travellers have hitherto overlooked. Mrs. Bishop (Miss Isabella L. Bird) proved equally attractive in describing her visit to the Bakhtiari country and the Karun River, and, as might have been expected, was somewhat more solid than her less-experienced fellow traveller.

The Ordnance Survey formed the subject of an important discussion in Section E, and the Association as a body has resolved to do its utmost to induce Government to introduce reforms. It is fortunate that by the combined action of Sections A, E, and G, a grant of £75 has been obtained for supplying instruments for climatological observations in Central Africa.

There was considerable discussion at the general committee meeting yesterday as to the date of the Edinburgh meeting next year. In certain quarters the end of September was advocated, but there can be no doubt that the great majority of the working members of the Association preferred the beginning of August, a date which will suit those connected with the Universities and will catch the citizens of Edinburgh before they leave for their holidays. It is, therefore, not surprising that August 3 has been fixed upon for the Edinburgh meeting, the President of which will be Sir Archibald Geikie. Nottingham has been selected as the place of meeting for 1893.

It is evident that the people of Cardiff are somewhat at a loss what to make of the Association and of the hundreds who are crowding the streets of the town and rushing from one Section room to another. The Sectional secretaries especially, seem to be a puzzle. In the hotel in which they are housed a commercial stock-room has been set apart for their use, with a long baize-covered table down the centre; while to discourage all tendencies to loafing, they have been provided with nothing else but hard kitchen chairs to sit upon.

Altogether, from a scientific point of view, the Cardiff meeting may be said to have come up to a fair average.

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. W. C. ROBERTS-AUSTEN, C.B., F.R.S., PRESIDENT OF THE SECTION.

THE selection of Cardiff as a place of meeting of the British Association led to the presidency of Section B being intrusted to a metallurgist. It will be well, therefore, to deal in this address mainly with considerations connected with the subject to which my life has been devoted, and I hope that it may be possible for me to show that this practical art has both promoted the advancement of science and has received splendid gifts in return.

It is an art for which in this country we have traditional love; nevertheless the modes of teaching it, and its influence on science, are but imperfectly understood and appreciated. Practical metallurgists are far too apt to think that improvements in their processes are mainly the result of their own experience and observation, unaided by pure science. On the other hand, those who teach metallurgy often forget that for the present they have not only to give instruction in the method of conducting technical operations, but have truly to educate, by teaching the chemistry of high temperatures, at which ordinary reactions are modified or even reversed, while they have further to deal with many phenomena of much importance, which cannot, as yet, be traced to the action of elements in fixed atomic proportions, or in which the direct influence of the atom is only beginning to be recognized.

The development of a particular art, like that of an organism, proceeds from its internal activity; it is work which promotes its growth and not the external influence of the environment. In the early stage of the development of an industry the craftsmen gather a store of facts which afford a basis for the labours of the investigator, who penetrates the circle of the "mystery" and renders knowledge scientific. Browning, inspired by the labours of a chemist, finely tells us in his "Paracelsus":—

> To know
> Rather consists in opening out a way
> Whence the imprisoned splendour may escape,
> Than in effecting entry for a light
> Supposed to be without.

If it be asked who did most in gaining the industrial treasure and in revealing the light of chemical knowledge, the answer is certainly the metallurgists, whose labours in this respect differ materially from others which have ministered to the welfare of mankind. First it may be urged that in no other art have the relations between theory and practice been so close and enduring. Bacon, who never undervalued research, tells us that in the division of the labour of investigation in the New Atlantis there are some "that raise the former discoveries by experiment into greater observations, axioms, and aphorisms: these we call the *interpreters of nature*." There are also others "that bend themselves, looking into the experiments of their fellows and casting about how to draw out of them things of use and practice for man's life and knowledge: . . . these we call the *dowry men or benefactors*." In reviewing the history of metallurgy, especially in our islands, it would seem that the two classes of workers, the interpreters of nature and the practical men, have for centuries sat in joint committee, and, by bringing theoretical speculation into close connection with hard industrial facts, have "carried us nearer the essence of truth."

The main theme of this address will therefore be the relation between theory and practice in metallurgy with special reference to the indebtedness of the practical man to the scientific investigator.

We will then consider—

(1) Certain facts connected with "oxidation" and "reduction," upon which depend operations of special importance to the metallurgist.

(2) The influence in metallurgical practice of reactions which are either limited or reversible.

(3) The means by which progress in the metallurgic art may be effected, and the special need for studying the molecular constitution of metals and alloys.

(1) The present year is a memorable one for chemists, being the centenary of the birth of Faraday and the bi-centenary of the death of Robert Boyle. The work of the former has recently been lovingly and fittingly dealt with in the Royal Institution, where he laboured so long. I would, in turn, briefly

recall the services of Boyle, not, however, on account of the coincidence of date, but because with him a new era in chemistry began. He knew too much about the marvellous action of "traces" of elements on masses of metal to feel justified in pronouncing absolutely against the possibilities of transmutation, but he did splendid service by sweeping away the firm belief that metals consist of sulphur, salt, and mercury, and by giving us the definition of an element. He recognized the preponderating influence of metallurgy in the early history of science, and quaintly tells us that "those addicted to chemistry have scarce any views but to the preparation of medicines or to the improvement of metals," a statement which was perfectly correct, for chemistry was built up on a therapeutic as well as a metallurgic basis. The fact is, however, that neither the preparation of materials to be employed in healing, nor the study of their action, had anything like the influence on the growth of theoretical chemistry which was exerted by a few simple metallurgical processes. Again, strange as it may seem, theoretical chemistry was more directly advanced by observations made in connection with methods of purifying the precious metals, and by the recognition of the quantitative significance of the results, than by the acquisition of facts incidentally gathered in the search for a transmuting agent. The belief that chemistry "grew out of alchemy" nevertheless prevails, and has found expression in this Section of the British Association. As a fact, however, the great metallurgists treated the search for a transmuting agent with contempt, and taught the necessity of investigation for its own sake. George Agricola, the most distinguished of the sixteenth century metallurgists, in his work "De Ortu et Causis Subterraneorum" (lib. v.), written about the year 1539, disdainfully rejects both the view of the alchemists that metals consist of sulphur and mercury, and their pretended ability to change silver into gold by the addition of foreign matter.

Biringuccio (1540) says, "I am one of those who ignore the art of the alchemists entirely. They mock nature when they say that with their medicines they correct its defects, and render imperfect metals perfect." "The art," he adds, "was not worthy of the consideration of the wise ancients who strove to obtain possible things." In his time, reaction between elements meant their destruction and reconstitution; nevertheless, his sentence "transmutation is impossible, because in order to transmute a body you must begin by destroying it altogether," suggests that he realized the great principle of the conservation of mass upon which the science of chemistry is based. We have also the testimony of the German metallurgist, Becher, who improved our tin-smelting in Cornwall. He is said to have caused a medal to be struck in 1675, which bore the legend, "Hanc unciam argenti finissimi ex plumbo arte alchymica transmutavi," though he should have been aware that he had only extracted the precious metal from the lead, and had not transmuted the base one. This is a lapse which must be forgiven him, for his *terra pinguis* was the basis of the theory of phlogiston, which exerted so profound an influence for a century after his death, and he wrote, "I wist that I have got hold of my pitcher by the right handle, for the pseudo-chemists seek gold, but I have the true philosophy, science, which is more precious."

At this critical period what was Boyle doing when the theory of phlogiston dawned in the mind of the metallurgist Becher? In 1672 Boyle wrote his paper on "Fire and Flame weighed in the Balance," and came to the conclusion that the "ponderous parts of flame" could pass through glass to get at melted lead contained in a closed vessel. It has been considered strange that he did not interpret the experiment correctly, but he, like the phlogistic chemists, tried to show that the *subtilis ignis*, the material of fire or phlogiston, would penetrate all things, and could be gained or lost by them. Moreover, his later experiments showed him that glass was powerless to screen iron from the "effluvium of a loadstone." His experiment with lead heated in a closed glass vessel was a fundamental one, to which his mind would naturally revert if he could come back now and review the present state of our knowledge in the light of the investigations which have been made in the two centuries that have passed since his own work ceased. If he turned to the end of the first century after his death he would see that the failure to appreciate the work of predecessors was as prevalent in the eighteenth century as in the sixteenth. The spirit of intolerance which lead Paracelsus to publicly burn, in his inaugural lecture at Basle, the works of Galen, Hippocrates, and Avicenna, urvived in the eighteenth century, when Madame Lavoisier burnt the works of Stahl, but it was reserved for the nineteenth century to reverently gather the ashes, recognizing that when the writers of the school of Becher spoke of phlogiston they meant what we understand by potential energy.

If Boyle, finding that the Fellows of the Royal Society had not carried out their intention to build a "Repository and Laboratory," sought the School of Mines and came to the Royal College of Science, he would surely thank my colleague, Prof. Thorpe, for his vigorous defence last year, as President of this Section, of the originality of the work of Priestley and Cavendish, to which Boyle's own researches had directly led. We on our part, remembering Berzelius's view that "oxygen is the centre point round which chemistry revolves," would hope to interest him most by selecting the experiments which arose out of the old metallurgical operation of separating the precious metals from lead by "cupellation." When, in conducting this operation, lead is heated in the presence of air, it becomes converted into a very fluid dross. Boyle had, in 1661, taken this operation as the very first illustration in his "Sceptical Chemist" in proof of his argument as to the elemental nature of metals. He would remember the quantitative work of Geber in the eighth century, who stated that the lead so heated in air acquired a "new weight," and he would appreciate the constant reference to the operation of cupellation from the close of the sixth century B.C., when the prophet Jeremiah wrote, to the work of Jean Rey in 1629, whose conclusions he would wish he had examined more closely. Lord Brouncker, as first President of the Royal Society, had called attention to the increase in weight of the lead in the "coppels" in the Assay Office in the Mint in the Tower, and Mayo had shown that the increase in weight comes from a distinct "*spiritus*" in the air. Boyle would incidentally see that Newton had accepted office in the Mint, where he doubtless continued his experiments on calcination, begun some time before, and, as if to mark his interest in the operation of assaying, figures are represented on a bas-relief on his tomb in Westminster Abbey as conducting cupellation in a muffle. The old work merges wonderfully into the new. Chevreul, in the nineteenth century, confirms Otto Tachens's view in the seventeenth, as to the saponifying action of litharge. Deville employs molten litharge to absorb oxygen dissociated from its compounds, and Graham, by extracting occluded gases from iron and other metals, proves the accuracy of the old belief that elastic fluids can freely permeate even solid metals.

We may imagine with what vivid interest Boyle would turn, not merely to the results of Priestley's work, but to his methods. Priestley had decomposed litharge with the electric spark, and had satisfied himself in 1774 by heating red lead that the gas he obtained in his earlier experiments was really the one now called oxygen.

Boyle would see, that in the period 1774-77 Lavoisier, being attracted by the "sceptical chemist's" own experiment on the heating of lead in closed vessels, overthrew the phlogistic theory, and placed chemistry on a firm basis by showing that the increase in weight of lead and tin, when heated in air, represents exactly the weight of the gaseous body added; and, finally, Dalton having developed the atomic theory and applied it to chemistry, Berzelius made lead memorable by selecting it for the first determination of an atomic weight.

Without diverting his attention from the phenomena of oxidation, Boyle would find questions the interest of which is only equalled by their present obscurity. He would contemplate the most interesting phase of the history of chemical science, described by Van 't Hoff as that of its evolution from the descriptive to the rational period, in the early days of which the impossibility of separating physics and chemistry became evident, and Boyle would find that chemistry is now regarded from the point of view of the mechanics of the atoms.

Deville's experiments on dissociation have rendered it possible to extend to the groups of atoms in chemical systems the laws which govern the fusion and vaporization of masses of matter, and this has produced a revolution comparable in its importance to that which followed the discovery of the law of definite proportions, for dissociation has shown us that true causes of chemical change are variations of pressure and of temperature. For instance, oxygen may be prepared on an industrial scale from air by the intervention of oxide of barium heated to a constant temperature of 700°, provided air be admitted to the heated oxide of barium, under a pressure of $1\frac{1}{2}$ atmospheres, while the oxygen, thus absorbed, is evolved if the containing vessel be rendered partially vacuous. It will be evident, therefore, that

at a certain critical temperature and pressure the slightest variation of either will destroy the equilibrium of the system and induce chemical change.

The aim of Boyle's chemical writings was to show that no barrier exists between physics and chemistry, and to "serve the commonwealth of learning by begetting a good understanding betwixt the chemists and the mechanical philosophers," who had, as he said, "been too great strangers to each other's discoveries." In view of the dominant lines of research which occupy chemists at the present time, such, for instance, as the investigations of "osmotic pressure" and of the application of Boyle's own law to salts in solution, he would feel that his hope had been realized, and that, though he lived a century too soon to take part in Berthollet's discussion with Proust, he nevertheless shares Berthollet's triumph in the long-delayed but now rapid development of chemistry as a branch of applied mechanics.

We need, however, no longer look at these questions from the point of view of Boyle, for our own interest in the application of chemical mechanics to metallurgy is sufficiently vivid, as instances to be given subsequently will show.

Hitherto I have mainly dwelt on questions relating to oxidation, but not less interesting is the history of the steps by which an accurate knowledge was acquired of the other great process practised by the metallurgist, the one to which Paracelsus was the first to apply the name of "reduction." Its explanation followed naturally from the elucidation of the phenomena of combustion by Lavoisier, who in continuation of Macquer's experiments of 1771 proved, in conjunction with other workers, that carbonic anhydride is produced when the diamond is burnt in air or oxygen. Carbon has been known for ages as the most important of the reducing agents, but when, in 1772, Lavoisier heated oxide of lead and carbon together, he did not at first recognize that carbonic anhydride had been produced, simply because the volume of the gas set free was the same as if oxygen merely had been liberated. He soon, however, saw that neither the carbon alone, nor the oxide of lead alone, gave rise to the evolution of carbonic anhydride, which resulted from the *mutual action* of carbon and a constituent of the litharge. "This last observation leads us insensibly," he adds, "to very important reflections on the use of carbon in the reduction of metals." It most certainly did, and by 1815 an accurate, if incomplete, view of reduction had passed into the encyclopædias. It was seen that the removal of oxygen from burnt metals, by carbon, "gives the metals," as Fourcroy and Vauquelin put it, "a new existence." Some ten years later Le Play attempted to show that reduction is always effected by the intervention of carbonic oxide, which elicited the classical rejoinder from Gay-Lussac, who pointed out that "carbon alone, and at very moderate temperatures, will reduce certain metallic oxides without the intervention of carbonic oxide or of any other elastic fluid." I mention these facts because metallurgists are slow to recognize their indebtedness to investigators, and too often ignore the extreme pains with which an accurate knowledge has been acquired of the principles upon which their processes have been based.

The importance of a coherent explanation of reduction in smelting pig-iron is enormous. The largest blast-furnaces in 1815 hardly exceeded those in use in the previous century, and were at most only 40 feet high, with a capacity of 5000 cubic feet. At the present day their gigantic successors are sometimes 90 feet high, with a capacity of 25,000 cubic feet. This development of the blast-furnace is due to the researches of a number of investigators, among whom von Tunner, Lowthian Bell, and Grüner deserve special mention. We are, however, forcibly reminded of the present incompleteness of our knowledge of the mechanism of reduction, when we remember that the experiments of H. P. Baker have led us to believe that pure carbon cannot be burnt in perfectly dry and pure oxygen, and therefore that the reducing agent, carbonic oxide, cannot be produced at all unless moisture be present.

Ludwig Mond, Langer, and Quincke, teach us not only that nickel can separate carbon from carbonic oxide, but the wholly unexpected fact that dry carbonic oxide can at a temperature of 100° take up nickel, which it again deposits if heated to 150°. Mond and Quincke, and, independently, Berthelot, have since proved the existence of the corresponding compound of iron and carbonic oxide, and it may safely be concluded that in the blast-furnace smelting iron this peculiar action of carbonic oxide plays an important part, and it doubtless aids the carburization of iron by cementation. It is truly remarkable that the past year should have brought us so great an increase in our knowledge of what takes place in the reduction of an oxide of iron, and in the carburization of the liberated metal. My own experiments have, I trust, made it clear that iron can, at an elevated temperature, be carburized by the diamond *in vacuo;* that is, in the absence of anything more than "a trace" of an elastic fluid or of any third element. Osmond has further shown within the last few months that the action between iron and carbon is a mutual one, for though carbon in the pure diamond form carburizes iron, the metal in its turn, at a temperature of 1050°, attacks the diamond, invests it with a black layer, and truly unites with it.

The question of the direct carburization of iron (Darby's process) by filtering the molten metal through carbon, promises to be of much importance, for at present, as is well known, two millions of tons of steel which are made in the Bessemer converter in this country alone, are re-carburized after "the blow" by the addition of spiegeleisen.

Carbonic oxide, moreover, would appear to be more chemically active than had been supposed; for during the present year Berthelot has shown that the perfectly pure gas heated to 500° or 550° produces carbonic anhydride with the deposition of carbon at red heat, not by ordinary dissociation, but by decomposition preceded by polymerization. He further shows that carbonic oxide will decompose ammoniacal nitrate of silver, and thus brings it into close connection with the aldehydes.

(2) In turning to the modern aspects of metallurgical practice, we shall see that the whole range of the metallurgist's field of study is changing. It is no longer possible for him to devise a series of operations on the evidence afforded by a set of equations which indicate the completion of an operation; he has, as I have already suggested, to consider the complicated problems which have been introduced into chemistry from the sciences of physics and mechanics. He has, in fact, no longer to deal merely with atoms and molecules, but with the influence of mass. As Ostwald points out, we are reminded that many chemical processes are reciprocating so that the original products may be obtained from the product of the reaction. The result of such opposed processes is a state of CHEMICAL EQUILIBRIUM, in which both the original and the newly-formed substances are present in definite quantities that remain the same so long as the conditions, more especially temperature and pressure, do not undergo further change. Again, in very many metallurgical processes, reactions are rendered incomplete by the limitations imposed by the presence of bodies which cannot be speedily eliminated from the system, and the result may be to greatly retard the completion of an operation. The time has come when the principles of dynamic chemistry must be applied to the study of metallurgical problems if they are to be correctly understood, and it is, moreover, necessary to remember the part played by the surface separating the different aggregates in contact with one another. When, for instance, a reaction has to take place accompanied by the evolution of gas, there must be space into which the gas can pass. The rate, therefore, at which change takes place will obviously depend on the state of division of the mass.

One of the most remarkable points in the whole range of chemistry is the action engendered between two elements capable of reacting by the presence of a third body. It may be, and this is the most wonderful fact of all, that merely a trace of a third body is necessary to induce reaction, or to profoundly modify the structure of a metal. H. Le Chatelier and Mouret have pointed out that in certain cases it is inaccurate to say that the third body causes the reaction to take place, because, after it has destroyed the inter-molecular resistances which prevented the reaction taking place, the third body ceases to intervene. This is apparently the case when platinum sponge effects the union of oxygen and hydrogen, or, conversely, when very hot platinum splits up water vapour into its constituent gases. Future investigation will, it is to be hoped, show whether the platinum does not exert some direct action in both cases. We can no longer neglect the study of such questions from the point of view of their practical application. The manufacture of red lead presents a case in point. In "drossing" molten lead, the oxidation of the lead is greatly promoted by the presence of a trace of antimony; and conversely, in the separation of silver from molten lead, by the aid of zinc, H. Roessler and Endelmann have recently shown that aluminium has a remarkable effect in protecting the zinc from loss by oxidation, and further, the presence of one-thousandth part of aluminium in

the zinc is sufficient to exert this protecting action on that metal. I am satisfied that if our metallurgists are to advance their industrial practice, they must, if I may use such an expression, persistently think in calories, and not merely employ the ordinary atomic "tools of thought." They will then be able to state what reactions can, under given conditions, take place; to indicate those which will be completed; and to avoid those that are impracticable.

In France, the country of so many great metallurgists, men like Le Chatelier and Ditte are doing admirable service, by bringing the results of the labours and teaching of St. Claire Deville within the range of practical men. And if I do not refer more specifically to their work it is for want of space and not of appreciation, but a few simple cases of reversible actions will perhaps make the subject clear. In the blast-furnace the main reducing agent, carbonic oxide, is produced from the solid fuel by the reaction $CO_2 + C = 2CO$, a reaction which is theoretically impossible because it is endothermic, and would be attended by absorption of heat. But heat external to the system intervenes, and acts either by depolymerizing the carbon into a simpler form which can combine with oxygen of the CO_2 with evolution of heat, or by dissociating carbonic anhydride sets oxygen free which combines with the carbon. Reduction of oxide of iron in the blast-furnace is mainly effected by carbonic oxide according to the well-known reaction

$$Fe_2O_3 + 3CO = 2Fe + 3CO_2.$$

But the gas issuing from a blast-furnace contains carbonic oxide, an important source of heat. The view that this loss of carbonic oxide was due to the fact that the contact of the ore and the reducing gas was not sufficiently prolonged, led to a great increase in the height of blast-furnaces, but without, as Grüner showed, diminishing the proportion of carbonic oxide escaping from the throat. The reduction of an iron ore by carbonic oxide only takes place within certain well-defined limits, and a knowledge of the laws of chemical equilibrium would have saved thousands and thousands of pounds which have been wasted in building unduly high furnaces. I would add that large sums have also been sacrificed in the vain attempt to smelt oxide of zinc in the blast-furnace, for which operation patents have frequently been sought, in ignorance or defiance of the readiness with which the inverse action occurs, so that the reducing action of carbon on oxide of zinc may be balanced by the re-oxidation of the reduced zinc by carbonic anhydride, which is the product of the reduction. A further instance may be borrowed from an electro-chemical process which has been adopted for obtaining alloys of aluminium. As is well known, all attempts to effect the direct reduction of alumina by carbon have failed, because the reaction

$$2(Al_2O_3) + 3C = 4Al + 3CO_2$$

requires 783·2 calories, while only 291 calories would result from the conversion of carbon into carbonic anhydride, therefore the reaction cannot be effected; but in Cowles's process aluminium is nevertheless liberated when alumina is mixed with charcoal and strongly heated by the passage of an electric current. This result is due, not to a simple reduction of alumina, but to its dissociation at the high temperature produced by the passage of a current of 1600 amperes between carbon poles, the liberated aluminium being at once removed from the system by metallic copper, which is simultaneously present and may not be without action itself.

An instance of the importance of these considerations is presented in the manufacture of steel by the basic process. Much care is devoted to obtaining conditions which will insure, not only the elimination, but the order of the disappearance of the impurities from the molten pig-iron. In the basic process as conducted in the closed converter, the phosphorus does not disappear until the carbon has left the fluid bath, whilst, when the open-hearth furnace is used, the elimination of the phosphorus may be effected before that of the carbon, and it is asserted that, if the carbon goes before the phosphorus is got rid of, a further addition of carbon is necessary. A curious and subtle case of chemical equilibrium is here presented. In the open-hearth furnace and Bessemer converter respectively, the temperatures and pressures are different, and the conditions as to the presentation of oxygen to the fluid bath are not the same. The result is that the relative rates of oxidation of the phosphorus and carbon are different in the two cases, although in either case, with a given method of working, there must be a ratio between the phosphorus and carbon in which they disappear simultaneously. The industrial bearing of the question is very remarkable. In the basic Bessemer process the tendency of the phosphorus to linger in the bath renders an "after-blow" necessary; it may be only of a few seconds' duration, but much iron is nevertheless burnt and wasted, and Mr. Gilchrist tells me that, if this after-blow could be avoided, a saving of some 6 per cent. of the yield of steel would be effected annually, the value of which, at the present rate of output and price of steel, is no less than a quarter of a million sterling.

The volatilization of sulphur in the converter while it is retained by the steel in the open-hearth furnace, and the increase in the percentage of manganese which leaves the slag and returns to the bath of metal in the converter at the end of the "blow," will probably be traced to the disturbance of equilibrium which attends very slight variations in the conditions, especially as regards temperature and pressure, under which the operations are conducted.

In the blast-furnace the reducing action must be greatly dependent on the rate at which alkaline cyanides are formed, and Hempel has recently shown, by the aid of well-devised experiments, that the quantity of cyanides which may be obtained at a high temperature from carbon, nitrogen, and alkaline oxides, increases as the pressure becomes greater.

Metallurgical chemistry is, in fact, a special branch of chemical science which does not come within the ordinary sphere of the academic teaching of chemistry. It is often urged that metallurgical practice depends upon the application of chemical principles which are well taught in every large centre of instruction in this country, but a long series of chemical reactions exist which are of vital importance to the metallurgist, though they are not set forth in any British manual of chemistry, nor are dealt with in courses of purely chemical lectures. I feel bound to insist upon this point, because, as Examiner in Metallurgy for the Science and Art Department, I find that purely analytical and laboratory methods are so often given in the belief that they are applicable to processes conducted on a large scale, and at high temperatures.

We are told that technical instruction should be kept apart from scientific education, which consists in preparing the student to apply the results of past experience in dealing with entirely new sets of conditions, but it can be shown that there is a whole side of metallurgical teaching which is truly educational, and leads students to acquire the habit of scientific thought as surely as the investigation of any other branch of knowledge.

It is, in fact, hardly possible in a course of theoretical chemistry to devote much attention to specific cases of industrial practice in which reactions are incomplete, because they are limited by the presence of bodies that cannot be directly eliminated from the chemical system. Take, for instance, the long series of reactions studied by Plattner, who published the results of his investigations in his celebrated treatise, "Die Metallurgische Röstprozesse," Freiberg, 1856, whose work I have chosen as a starting-point on account of our presence in South Wales near the great copper-smelting district of Swansea. A complex sulphide, of which copper is the main metallic constituent, contains some fifty ounces of silver to the ton. The problem may be supposed for the present to be limited to the extraction of the precious metal from the mass in which it is hidden, and the student deriving his knowledge from an excellent modern chemical treatise would find the case thus stated:—

"Ziervogel's process depends upon the fact that when argentiferous copper pyrites is roasted, the copper and iron sulphides are converted into insoluble oxides, whilst the silver is converted into a soluble sulphate, which is dissolved out by lixiviating the roasted ore with hot water, the silver being readily precipitated from this solution in the metallic state."

It is certain that if an observant, chemically-trained student visited a silver extraction works, and possessed sufficient analytical skill to enable him to secure evidence as to the changes that occur, he would find a set of facts which his training had not enabled him to predict, and he would establish the existence of a set of reactions to the nature of which his chemical reading had hardly given him a clue. The process to be considered is a simple one, but it is typical, and applies to a large proportion of the 7,000,000 ounces of silver annually obtained in the world from cupriferous compounds. He would be confronted with a ton or more of finely-divided material spread in a thin layer over

the bed of a reverberatory furnace. Suppose the material is what is known as a complex regulus, as imported into Swansea or produced at Freiberg, to which are added rich native sulphides. The mixture then consists of sulphides mainly of iron and copper, with some sulphide of lead, and contains fifty or sixty ounces of silver to the ton, and a few grains of gold. It may also contain small quantities of arsenic and antimony as arsenides, antimonides, and sulpho-salts, usually with copper as a base.

The temperature of the furnace in which the operation is to be performed is gradually raised, the atmosphere being an oxidizing one. The first effect of the elevation of the temperature is to distil off sulphur, reducing the sulphides to a lower stage of sulphurization. This sulphur burns in the furnace atmosphere to sulphurous anhydride (SO_2), and, coming in contact with the material undergoing oxidation, is converted into sulphuric anhydride (SO_3). It should be noted that the material of the brickwork does not intervene in the reactions, except by its presence as a hot porous mass, but its influence is, nevertheless, considerable. The roasting of these sulphides presents a good case for the study of chemical equilibrium. As soon as the sulphurous anhydride reaches a certain tension, the oxidation of the sulphide is arrested, even though an excess of oxygen be present, and the oxidation is not resumed until the action of the draught changes the conditions of the atmosphere of the furnace, when the lower sulphides remaining are slowly oxidized, the copper sulphide being converted into copper sulphate mainly by the intervention of the sulphuric anhydride formed as indicated. Probably by far the greater part of the iron sulphide only becomes sulphate for a very brief period, being decomposed into the oxides of iron, mainly ferric oxide, the sulphur passing off. Any silver sulphide that is present would have been converted into metallic silver at the outset were it not for the simultaneous presence of other sulphides, notably those of copper and of iron, which enables the silver sulphide to become converted into sulphate. The lead sulphide is also converted into sulphate at this low temperature. The heat is now raised still further with a view to split up the sulphate of copper, the decomposition of which leaves oxide of copper. If, as in this case, the bases are weak, the sulphuric anhydride escapes mainly as such; but when the sulphates of stronger bases are decomposed, the sulphuric anhydride is to a great extent decomposed into a mixture of sulphurous anhydride and oxygen. The sulphuric anhydride, resulting from the decomposition of this copper sulphate, converts the silver into sulphate, and maintains it as such, just as, in turn, at a lower temperature, the copper itself had been maintained in the form of sulphate by the sulphuric anhydride eliminated from the iron sulphide. When only a little of the copper sulphate remains undecomposed, the silver sulphate begins to split up, and the furnace charge must therefore be immediately withdrawn, or the whole of the silver sulphate would be converted into metallic silver, partly by the direct action of heat alone and partly by reactions such as those shown in the following equations:—

$$Ag_2SO_4 + 4Fe_3O_4 = 2Ag + 6Fe_2O_3 + SO_2,$$
$$Ag_2SO_4 + Cu_2O = 2Ag + CuSO_4 + CuO.$$

If the charge were not withdrawn, the silver would thus be effectually removed from the solvent action of water, and the smelter's efforts would have failed entirely. The charge still contains lead sulphate, which cannot be completely decomposed at any temperature attainable in the roasting furnace, except in the presence of silica, and it is well to leave it where it is if the residue has subsequently to be smelted with a view to the extraction of the gold. The elimination of arsenic and antimony gives rise to problems of much interest, and again confronts the smelter with a case of chemical equilibrium. For the sake of brevity it will be well for the present to limit the consideration to the removal of antimony, which may be supposed to be present as sulphide. Some sulphide of antimony is distilled off, but this is not its only mode of escape. An attempt to remove antimony by rapid oxidation would be attended with the danger of converting it into insoluble antimoniates of the metals present in the charge. In the early stages of the roasting it is therefore necessary to employ a very low temperature, and the presence of steam is found to be useful as a source of hydrogen, which removes sulphur as hydrogen sulphide, the gas being freely evolved. The reaction

$$Sb_2S_3 + 3H_2 = 3H_2S + 2Sb$$

between hydrogen and sulphide of antimony is, however, endothermic, and could not, therefore, take place without the aid which is afforded by external heat. The facts appear to be as follows: sulphide of antimony, when heated, dissociates, and the tension of the sulphur vapour would produce a state of equilibrium if the sulphur thus liberated were not seized by the hydrogen and removed from the system. The equilibrium is thus destroyed, and fresh sulphide is dissociated; the general result being that the equilibrium of the system is continually restored and destroyed until the sulphide is decomposed. The antimony combines with oxygen, and escapes as volatile oxide, as does also the arsenic, a portion of which is volatilized as sulphide.

The main object of the process which has been considered is the formation of soluble sulphate of silver. If arsenic and antimony have not been eliminated, their presence at the end of the operation would be specially inconvenient, as they give rise to the formation of arseniate and antimoniate of silver, insoluble in water, which may necessitate the treatment of the residues by an entirely different process from that which has hitherto been considered.

It will have been evident that effecting this series of changes demands the exercise of the utmost skill, care, and patience. The operations beginning at a dull red heat, or a temperature of some 500°, are completed at 700°, within a range, that is, of 200°. Judicious stirring has been necessary to prevent the formation of crusts of sulphates, which would impede the reactions, and, as has been shown, an undue elevation of temperature within a very limited range would, at any stage, have been fatal to the success of the operation. It is difficult to appreciate too highly the delicacy of sight and touch which enables an operator to judge by the aid of rough tests, but mainly from the tint of the streak revealed when the mass is rabbled, whether any particular stage has or has not been reached, and it will be obvious that the requisite skill is acquired solely by observation and experiment. The technical instructor may impart information as to the routine to be followed, and the appearances to be observed, but scientific knowledge of a high order can alone enable the operator to contend with the disturbing influences introduced by the presence of unexpected elements or by untoward variations in temperature. In the training of a metallurgist it is impossible to separate education from instruction, and the above description of a very ordinary operation will show the intimate relations between science and practice which are characteristic of metallurgical operations. Practice is dependent on science for its advancemement, but scientific workers too often hesitate to attack metallurgical problems, and to devote the resources of modern investigation to their solution, because they are not aware of the great interest of the physical and chemical problems which are connected with many very simple metallurgical processes, especially with those that are conducted at high temperatures.

Proceeding yet one step further, suppose that the copper-smelter takes possession of the residual mass, consisting mainly of oxide of copper, he would smelt it with fresh sulphide ores, and obtain, as a slag from the earthy matters of the ore, a ferrous silicate containing some small proportion of copper. The displacement of the copper from this silicate may be effected by fusing it with sulphide of iron, a fusible sulphide of iron and copper being formed, which readily separates from the slag. By this reaction some twenty thousand tons of copper are added to the world's annual production. Proceeding yet a step further, suppose the smelter to have reduced his copper to the metallic state. If arsenic had been originally present in the ore, and had not been eliminated entirely in the roasting, extraordinary difficulties will be met with in the later stages of the process, in extracting small quantities of arsenic which resist the smelter's efforts. Copper, moreover, containing arsenic cannot be "overpoled," as the presence of arsenic hinders the reducing action of gases on the copper. The amount of arsenic which the copper-smelter has to remove may vary from mere traces up to 1 per cent., and if the copper is destined for the use of the electrical engineer, he will insist on its being as pure as possible, for the presence of a trace of arsenic would materially increase the electrical resistance of the copper, and would be fatal to its use in submarine telegraphy. If, on the other hand, the copper is intended for the maker of locomotive fire-boxes, he will encourage the retention of small quantities of arsenic, as it is found to actually increase the endurance of the copper, and the smelter will in such a case have no inducement to employ the basic furnace lining which Mr. Gilchrist has offered him, nor will he care to use the special methods for the removal of arsenic

with which he is familiar. It may all seem simple enough, but the modern process of copper-smelting has been laboriously built up, and has a long and interesting pedigree which may be traced to at least the eighth century, when Geber described the regulus, "coarse metal," as being "black mixed with livid," and our familiar "blue metal" as being "of a most clean and pleasant violet colour," and indicated the reason for the difference.[1]

(3) The foregoing instances have been given to indicate the general nature of metallurgical chemistry. It will be well now to show how the great advances in metallurgical practice have been made in the past, with a view to ascertain what principles should guide us in the future.

It is a grave mistake to suppose that in industry, any more than in art, national advance takes place always under the guidance of a master possessed of some new gift of invention; yet we have been reminded that we are apt to be reverent to these alone, as if the nation had been unprogressive and suddenly awakened by the genius of one man. The way for any great technical advance is prepared by the patient acquisition of facts by investigators of pure science. Whether the investigators are few or many, and consequently whether progress is slow or rapid, will depend in no small measure on the spirit of the nation as a whole. A genius whose practical order of mind enables him to make some great invention suddenly arises, apparently by chance, but his coming will, in most cases, be found to have "followed hard upon" the discovery by some scientific worker of an important fact, or even the accurate determination of a set of physical constants. No elaborate monograph need have reached the practical man—a newspaper paragraph, or a lecture at a Mechanics' Institute may have been sufficient to give him the necessary impulse; but the possessors of minds which are essentially practical often forget how valuable to them have been the fragments of knowledge they have so insensibly acquired that they are almost unconscious of having received any external aid.

The investigating and the industrial faculty are sometimes, though rarely, united in one individual. Rapid advance is often made by those who are untrammelled by a burden of precedent, but it should be remembered that though the few successes, which have been attained in the course of ignorant practice, may come into prominence, none of the countless failures are seen.

I would briefly direct attention to certain processes which have been adopted since the year 1849, when Dr. Percy presided over this Section at Birmingham, a great metallurgical centre. In that year the President of the Association made a reference to metallurgy, a very brief one, for Dr. Robinson only said "the manufacture of iron has been augmented six-fold by the use of the puddling-furnace and the hot-blast, both gifts of theory"; and so, it may be added, are most of the important processes which have since been devised. Take the greatest metallurgical advance of all, the Bessemer process, which has probably done more than any other to promote the material advance of all countries. It was first communicated to the world at the Cheltenham Meeting of the British Association, 1856. Its nature is well known, and I need only say that it depends on the fact that when air is blown through a bath of impure molten iron, sufficient heat is evolved by the rapid combustion of silicon, manganese, and carbon, to maintain the bath fluid after these elements have been eliminated, there being no external source of heat, as there is in the puddling furnace or the refinery hearth. We have recently been told that, at an early and perilous stage of the Bessemer process, confidence in the experiments was restored by the observation that the temperature of the "blown" metal contained in a crucible was higher than that of the furnace in which it was placed. The historian of the future will not fail to record that the way for the Bessemer process had been prepared by the theoretical work of Andrews, 1848, and of Favre and Silbermann, 1852, whose work on the calorific power of various elements showed that silicon and phosphorus might be utilized as fuel, because great heat is engendered by their combustion.

The basic process for removing phosphorus, a process of great national importance, the development of which we owe to Thomas and Gilchrist, is entirely the outcome of purely theoretical teaching, in connection with which the names of Grüner and Percy deserve special mention. In the other great group of processes for the production of steel, those in which Siemens's regenerative furnace is employed, we have the direct influence of a highly trained theorist, who concluded his address as President of this Association in 1882 by reminding us that "in the great workshop of Nature there is no line of demarcation to be drawn between the most exalted speculation and commonplace practice." The recent introduction of the method of heating by radiation is, of course, the result of purely theoretical considerations.

The progress in the methods of extracting the precious metals has been very great, both on the chemical and engineering sides, but it is curious that in the metallurgy of gold and silver, many ancient processes survived which were arrived at empirically—a noteworthy exception being presented by the chlorine process for refining gold, by the aid of which many millions sterling of gold have been purified. The late Mr. H. B. Miller based this process for separating silver from gold on the knowledge of the fact that chloride of gold cannot exist at a bright red heat. The tension of dissociation of chloride of gold is high, but the precious metal is not carried forward by the gaseous stream, at least not while chloride of silver is being formed.

The influence of scientific investigation is, however, more evident in that portion of the metallurgic art which deals with the adaptation of metals for use, rather than with their actual extraction from the ores.

Only sixteen years ago Sir Nathaniel Barnaby, then Director of Naval Construction, wrote, "Our distrust of steel is so great that the material may be said to be altogether unused by private ship-builders, . and marine engineers appear to be equally afraid of it." He adds, "The question we have to put to the steel makers is, What are our prospects of obtaining a material which we can use without such delicate manipulation and so much fear and trembling?" All this is changed, for, as Mr. Elgar informs me, in the year ending on June 30 last, no less than 401 ships, of three-quarters of a million gross tonnage, were being built of steel in the United Kingdom.

Why is it, then, that steel has become the material on which we rely for our ships and for our national defence, and of which such a splendid structure as the Forth Bridge is constructed? It is because, side by side with great improvement in the quality of certain varieties of steel, which is the result of using the open-hearth process, elaborate researches have shown what is the most suitable mechanical and thermal treatment for the metal; but the adaptation of steel for industrial use is only typical, as the interest in this branch of metallurgy generally appears for the moment to be centred in the question whether metals can, like many metalloids, pass under the application of heat or mechanical stress from a normal state to an allotropic one, or whether metals may even exist in numerous isomeric states.

It is impossible to deal historically with the subject now, further than by stating that the belief of more than one "modification" is old and widespread, and was expressed by Paracelsus, who thought that copper "contains in itself its female," which could be isolated so as to give "two metals" . . . "different in their fusion and malleability" as steel and iron differ. Within the last few years Schützenberger has shown that two modifications of copper can exist, the normal one having a density of 8·95, while that of the allotropic modification is only 8·0, and is moreover rapidly attacked by dilute nitric acid, which is without action on ordinary copper. It may be added that Lord Rayleigh's plea for the investigation of the simpler chemical reactions has been partly met, in the case of copper, by the experiments conducted by V. H. Veley on the conditions of chemical change between nitric acid and certain metals.

Bergmann, 1781, actually calls iron polymorphous, and says that it plays the part of many metals, "Adeo ut jure dici queat polymorphum ferrum plurium simul metallorum vices sustinere." Osmond has recently demonstrated the fact that at least two modifications of iron must exist.

Prof. Spring, of Liége, has given evidence that in cooling

[1] It must not be supposed that when commercially pure copper lies on the furnace bed, ready to be transferred to moulds, that its turbulent career of reactions is over. It might be thought that the few tenths per cent. of impurity, dissolved oxide, and occluded gas, are so far attenuated by distribution that their interactions must be insignificant. This is far from being the case. I believe the bath of metal is seething from its reactions until the copper is solid, and then polymerization proceeds. There may not be a sharply-defined, critical range of temperature within which the metal can alone be successfully worked, and which varies, as regards its starting-point, with the kind of impurity present, as is the case with steel; but evidence of molecular change in the solid metal is afforded by the pyrometric curves of cooling referred to on p. 405, and by the singular behaviour as regards electrical resistance, of various samples of copper, in which chemical analysis hardly reveals a difference.

lead-tin alloys polymerization may take place after the alloys have become solid, and it seems to be admitted that the same cause underlies both polymerization and allotropy. The phenomenon of allotropy is dependent upon the number of the atoms in each molecule, but we are at present far from being able to say what degree of importance is to be attached to the relative distance between the atoms of a metal or to the "position of one and the same atom" in a metallic molecule, whether the metal be alloyed or free, and it must be admitted that in this respect organic chemistry is far in advance of metallurgic chemistry. I cannot, as yet, state what is the atomic grouping in the brilliantly-coloured gold-aluminium alloy, $AuAl_2$, which I have had the good fortune to discover, but, in it, the gold is probably present in the same state as that in which it occurs in the purple of cassius.

Much valuable information on the important question of allotropy in metals has already been gathered by Pionchon, Ditte, Moissan, Le Chatelier, and Osmond, but reference can only be made to the work of the two latter. Le Chatelier concludes that in metals which do not undergo molecular transformation the electrical resistance increases proportionally to the temperature. The same law holds good for other metals at temperatures above that at which their last change takes place; for example, in the case of nickel above 340°, and in that of iron above 850°.

It is probable that minute quantities of foreign matter, which profoundly modify the structure of masses of metal, also induce allotropic changes. In the case of the remarkable action of impurities upon pure gold I have suggested that the modifications which are produced may have direct connection with the periodic law of Mendeleeff, and that the causes of the specific variations in the properties of iron and steel may thus be explained. The question is of great industrial importance, especially in the case of iron; and Osmond, whose excellent work I have already brought before the members of this Association in a lecture delivered at Newcastle in 1889, has especially studied the influence upon iron exerted by certain elements. He shows that elements whose atomic volumes are smaller than that of iron delay, during the cooling of a mass of iron from a red heat, the change of the β, or hard variety of iron, to the α, or soft variety. On the other hand, elements whose atomic volumes are greater than that of iron tend to hasten the change of β to α iron. It is, however, unnecessary to dwell upon this subject, as it was dealt with last year in the address of the President of the Association.

It may be added that the recent use of nickel-steel for armour-plate, and the advocacy of the use of copper-steel for certain purposes, is the industrial justification of my own views as to the influence of the atomic volume of an added element on the mechanical properties of iron, and it is remarkable that the two bodies, silicon and aluminium, the properties of which when in a free state are so totally different, should, nevertheless, when they are alloyed with iron, affect it in the same way. Silicon and aluminium have almost the same atomic volumes.

The consequences of allotropic changes which result in alteration of structure are very great. The case of the tin regimental buttons which fell into a shapeless heap when exposed to the rigorous winter at St. Petersburg, is well known. The recent remarkable discovery by Hopkinson of the changes in the density of nickel-steel (containing 22 per cent. of nickel) which are produced by cooling to − 30°, affords another instance. This variety of steel, after being frozen, is readily magnetizable, although it was not so before; its density, moreover, is permanently reduced by no less than 2 per cent. by the exposure to cold; and it is startling to contemplate the effect which would be produced by a visit to the Arctic regions of a ship of war built in a temperate climate of ordinary steel, and clad with some three thousand tons of such nickel-steel armour; the shearing which would result from the expansion of the armour by exposure to cold would destroy the ship. Experimental compound armour-plates have been made, faced with 25 per cent. nickel-steel, but it remains to be seen whether a similar though lessened effect would be produced on the steel containing 5 to 7 per cent. of nickel, specially studied by J. Riley, the use of which is warmly advocated for defensive purposes. Further information as to the molecular condition of nickel-steel has within the last few weeks been given by Mercadier, who has shown that alloying iron with 25 per cent. of nickel renders the metal isotropic.

The molecular behaviour of alloys is indeed most interesting. W. Spring has shown, in a long series of investigations, that alloys may be formed at the ordinary temperature, provided that minute particles of the constituent metals are submitted to great pressure. W. Hallock has recently given strong evidence in favour of the view that an alloy can be produced from its constituent metals with but slight pressure if the temperature to which the mass is submitted be above the melting-point of the alloy, even though it be far below the melting-point of the most easily fusible constituent. A further instance is thus afforded of the fact that a variation of either temperature or pressure will effect the union of solids. It may be added that B. C. Damien is attempting to determine what variation in the melting-point of alloys is produced by fusing them under a pressure of two hundred atmospheres. Italian physicists are also working on the compressibility of metals, and F. Boggio-Lera has recently established the existence of an interesting relation between the coefficient of cubic compressibility, the specific gravity, and the atomic weight of metals.

Few questions are more important than the measurement of very high temperatures. Within the last few years H. Le Chatelier has given us a thermo-couple of platinum with platinum containing 10 per cent. of rhodium, by the aid of which the problem of the measurement of high temperatures has been greatly simplified. A trustworthy pyrometer is now at hand for daily use in works, and the liberality of the Institution of Mechanical Engineers has enabled me to conduct an investigation which has resulted in the adoption of a simple appliance for obtaining, in the form of curves, photographic records of the cooling of masses of metal. A report on the subject has already been submitted to a Committee, of which the Director-General of Ordnance Factories is the Chairman; and Dr. Anderson, to whom I am indebted for valuable assistance and advice, intends to add this new method for obtaining autographic curves of pyrometric measurements to the numerous self-recording appliances used in the Government factories which he controls. It has proved to be easy to ascertain, by the aid of this pyrometer, what thermal changes take place during the cooling of molten masses of alloys, and it is possible to compare the rate of cooling of a white-hot steel ingot at definite positions situated respectively near its surface and at its centre, and thus to solve a problem which has hitherto been considered to be beyond the range of ordinary experimental methods. Some of the curves already obtained are of much interest, and will be submitted to the Section. It is probable that the form of the curve which represents the solidification and cooling of a mass of molten metal affords an exceedingly delicate indication as to its purity.

Prof. H. E. Armstrong holds that the molecules of a metal can unite to form complexes with powers of coherence which vary with the presence of impurity. Crookes, by a recent beautiful investigation, has taught us how electrical evaporation of solid metals may be set up *in vacuo*, and has shown that even an alloy may be decomposed by such means. We may hope that such work will enable us to understand the principles on which the strength of materials depends.

Before leaving the consideration of questions connected with the molecular constitution of metals, I would specially refer to the excellent work of Heycock and Neville, who have extended to certain metals with low melting-point Raoult's investigations on the effect of impurity on the lowering of the freezing-point of solids. With the aid of one of my own students, H. C. Jenkins, I have further extended the experiments by studying the effect of impurity on the freezing-point of gold. Ramsay, by adopting Raoult's vapour-pressure method, has been led to the conclusion that when in solution in mercury the atom of a metal is, as a rule, identical with its molecule. The important research on the liquation of alloys has been extended by E. Matthey to the platinum-gold and palladium-gold series, in which the manipulation presented many difficulties; and E. J. Ball has studied the cases presented by the antimony copper-lead series. Dr. Alder Wright has continued his own important investigation upon ternary alloys; and A. P. Laurie has worked on the electromotive force of the copper-zinc and copper-tin and gold-tin series, a field of research which promises fruitful results.

In no direction is advance more marked than in the mechanical testing of metals, in which branch of investigation this country, guided by Kirkaldy, undoubtedly took the leading part, and in connection with which Kennedy and Unwin have established world-wide reputations. I would also specially mention the work which has been carried on at the Government testing works at Berlin under Dr. Wedding, and the elaborate

investigations conducted at the Watertown Arsenal, Massachusetts, not to mention the numerous Continental testing laboratories directed by such men as Bauschinger, Jenny, and Tetmajer. Perhaps the most important recent work is that described by Prof. Martens, of Berlin, on the influence of heat on the strength of iron.

I might have dwelt at length on all these matters without doing half the service to metallurgy that I hope to render by earnestly pleading for the more extended teaching of the subject throughout the country, and for better laboratories, arranged on the model of engineering laboratories, in which the teaching is conducted with the aid of complete, though small, "plant." The Science and Art Department has done great and lasting service by directing that metallurgy shall be taught practically, but much remains to be done. With regard to laboratories in works, which are too often mere sheds, placed, say, behind the boiler-house, when may we hope to rival the German chemical firm which has recently spent £19,000 upon its laboratories, in which research will be vigorously conducted? There is hardly any branch of inorganic chemistry which the metallurgist can afford to neglect, while many branches both of physics and mechanics are of the utmost importance to him.

The wide range of study upon which a metallurgical student is rightly expected to enter is leading, it is to be feared, to diminution in the time devoted to analytical chemistry, and this most serious question should be pressed upon the attention of all who are responsible for the training of our future chemists. There can be no question that sufficient importance is not attached to the estimation of "traces," an analysis being considered to be satisfactory if the constituents found add up to 99·9, although a knowledge as to what elements represent the missing 0·1 may be more useful in affording an explanation of the defects in a material than all the rest of the analysis. This matter is of growing interest to practical men, and may explain their marked preference for chemists who have been trained in works, to those who have been educated in a college laboratory.

The necessity for affording public instruction in mining and metallurgy, with a view to the full development of the mineral wealth of a nation, is well known. The issues at stake are so vast, that in this country it was considered desirable to provide a centre of instruction in which the teaching of mining and metallurgy should not be left to private enterprise or even intrusted to a corporation, but should be under the direct control of the Government. With this end in view, the Royal School of Mines was founded in 1851, and has supplied a body of well-trained men who have done excellent service for the country and her colonies. The Government has recently taken a step in advance, and has further recognized the national importance of the teaching of mining and metallurgy by directing that the School of Mines shall be incorporated with the Royal College of Science, which is, I believe, destined to lead the scientific education of the nation.

It is to be feared that as regards metalliferous mining our country has seen its best days, but the extraordinary mineral wealth of our colonies has recently been admirably described by my colleague, Prof. Le Neve Foster, in the inaugural lecture he delivered early in the present year on his appointment to the chair so long held by Sir Warington Smyth (*Engineering*, vol. li., p. 200 *et seq.*). We shall, however, be able to rightly estimate the value of our birthright when the Imperial Institute is opened next year, and the nation will have reason to be grateful to Sir Frederick Abel for the care he is devoting to the development of this great institution, which will become the visible exponent of the splendours of our Indian and colonial resources, as well as a centre of information.

The rapid growth of technical literature renders it unnecessary for a President of a Section to devote his address to recording the progress of the subject he represents. As regards the most important part of our national metallurgy, this has, moreover, been admirably done by successive Presidents of the Iron and Steel Institute, but it may have been expected that references would have been made to the main processes which have been adopted since Percy occupied this chair in 1849. I have not done so, because an enumeration of the processes would have been wholly inadequate, and a description of them impossible in the time at my disposal. Nevertheless, it may be well to remind the Section of a few of the more prominent additions the art has received in the last half-century, and to offer a few statements to show the magnitude on which operations are conducted. As regards iron, in the last twenty-five years the price of steel has been reduced from £55 per ton to £5 per ton; but, after giving the world the inestimable boon of cheap steel by the labours of Bessemer and of Siemens, we were somewhat slow to accept the teaching of experiment as to the best method of treating the new material; on the other hand, Hadfield has brought manganese steel and aluminium steel within the reach of the manufacturer, and J. Riley has done much to develop the use of nickel-steel.

In the case of copper, we have mainly contributed to the extraordinary development of wet processes for its extraction from poor sulphides, and have met the great demands for pure metal by the wide adoption of electrolytic processes.

As regards the precious metals, this country is well to the front, for Great Britain and her colonies produce about 38 per cent. of the gold supply of the world; and it may be well to add, as an indication of the scale on which operations are conducted, that in London alone one ton of gold and five tons of silver bullion can easily be refined in a day. No pains have been spared in perfecting the method of assay by which the value of gold and silver is ascertained, and during my twenty years' connection with the Royal Mint I have been responsible for the accuracy of the standard fineness of no less than five hundred and fifty-five tons of gold coin, of an aggregate value of seventy millions five hundred thousand pounds sterling. In the case of the platinum industry we owe its extraordinary development to the skill and enterprise of successive members of the firm of Johnson, Matthey, and Co., who in later years have based their operations upon the results of the investigations of Deville and Debray. Some indication of the value of the material dealt with may be gathered from the statement that two and a half hundredweight of platinum may easily be melted in a single charge, and that the firm, in one operation, extracted a mass of palladium valued at £30,000 from gold-platinum ore actually worth more than a million sterling.

I wish it were possible to record the services of those who have advanced metallurgy in connection with this Association, but the limitations of time render it difficult to do more than to refer to some honoured names of past presidents of this Section. Michael Faraday, President of this Section in 1837 and 1846, prepared the first specimen of nickel-steel, an alloy which seems to have so promising a future, but we may hardly claim him as a metallurgist; nor should I be justified in referring, in connection with metallurgical research, to my own master, Graham, President of this Section in 1839, and again in 1844, were it not that his experiments on the occlusion of gases by metals have proved to be of such extraordinary practical importance in connection with the metallurgy of iron. Sir Lyon Playfair presided over this Section in 1855, and again in 1859. His work in connection with Bunsen on the composition of blast-furnace gases was published in the Report of this Association in 1847, and formed the earliest of a group of researches, amongst which those of Sir Lowthian Bell proved to be of so much importance. The latter was President of this Section in 1889. Sir F. Abel, President of this Section in 1877, rendered enduring service to the Government by his elaborate metallurgical investigations in connection with materials used for guns and projectiles, as well as for defensive purposes. I will conclude this section of the address by a tribute to the memory of Percy. He may be said to have created the English literature of metallurgy, to have enriched it with the records of his own observations, and to have revived the love of our countrymen for metallurgical investigation. His valuable collection of specimens, made while Professor at the Royal School of Mines, is now appropriately lodged at South Kensington, and will form a lasting memorial of his labours as a teacher. He exerted very noteworthy influence in guiding the public to a just appreciation of the labours of scientific men, and he lived to see an entire change in the tone of the public press in this respect. In the year of Percy's presidency over this Section the *Times* gave only one-tenth of a column to a summary of the results of the last day but one of the meeting, although the usual discourse delivered on the previous evening had been devoted to a question of great importance—"The Application of Iron to Railway Purposes." Space was, however, found for the interesting statement that the "number of Quakeresses who attended the meetings of the Sections was not a little remarkable." Compare the slender record of the *Times* of 1849 with its careful chronicle of the proceedings at any recent meeting of the Association.

In drawing this address to a close, I would point to the great importance of extending the use of the less known metals. Attention is at present concentrated on the production of aluminium, and reference has already been made to the Cowles process, in which, as in that of Héroult, the reduction of alumina is effected by carbon, at the very high temperature of the electric arc; while, on the other hand, in the Kleiner and similar processes, the electric current acts less as a source of heat than by decomposing a fluid bath, the aluminium being isolated by electrolytic action; and doubtless in the immediate future, there will be a rapid increase in the number of metallurgical processes that depend on reactions which are set up by submitting chemical systems to electric stress. Incidental reference should be made to the growing importance of sodium, not only in cheapening the production of aluminium, but as a powerful weapon of research. In 1849, when Percy was President of this Section, magnesium was a curiosity; now its production constitutes a considerable industry. We may confidently expect to see barium and calcium produced on a large scale as soon as their utility has been demonstrated by research. Minerals containing molybdenum are not rare; and the metal could probably be produced as cheaply as tin if a use were to be found for it. The quantities of vanadium and thallium which are available are also far from inconsiderable; but we as yet know little of the action of any of these metals when alloyed with others which are in daily use. The field for investigation is vast indeed, for it must be remembered that valuable qualities may be conferred on a mass of metal by a very small quantity of another element. The useful qualities imparted to platinum by iridium are well known. A small quantity of tellurium obliterates the crystalline structure of bismuth; but we have lost an ancient art, which enabled brittle antimony to be cast into useful vessels. Two-tenths per cent. of zirconium increases the strength of gold enormously, while the same amount of bismuth reduces the tenacity to a very low point. Chromium, cobalt, tungsten, titanium, cadmium, zirconium, and lithium are already well known in the arts, and the valuable properties which metallic chromium and tungsten confer upon steel are beginning to be generally recognized, as the last Exhibition at Paris abundantly showed; but as isolated metals we know but little of them. Is the development of the rarer metals to be left to other countries? Means for the prosecution of research are forthcoming, and a rich reward awaits the labours of chemists who could bring themselves to divert their attention, for even a brief period, from the investigation of organic compounds, in order to raise alloys from the obscurity in which they are at present left.

It must not be forgotten that metallurgical enterprise rests on (1) scientific knowledge, (2) capital, and (3) labour; and that, if the results of industrial operations are to prove remunerative, much must depend on the relation of these three elements, though it is difficult to determine accurately their relative importance. A modern ironworks may have an army of ten thousand workmen, and commercial success or failure will depend in no small measure on the method adopted in organizing the labour. The relations between capital and labour are of so much interest at the present time that I do not hesitate to offer a few words on the subject.

Many examples might be borrowed from metallurgical enterprises in this and other countries to show that their nature is often precarious, and that failure is easily induced by what appear to be comparatively slight causes. Capitalists might consequently tend to select Government securities for investment in preference to metallurgical works, and the labouring population would then severely suffer. It is only reasonable, therefore, that if capitalists are exposed to great risks, they should, in the event of success, receive the greater part of the profits. There is a widespread feeling that the interests of capital and labour must be antagonistic, and as it is impossible to ignore the fact that the conflict between them is giving rise to grave apprehension, it becomes the duty of all who possess influence to strive not merely for peace, but to range themselves on the side of justice and humanity. The great labour question cannot be solved except by assuming as a principle that private ownership must be held inviolable; but it must be admitted that there was a time when capital had become arbitrary, and some kind of united action on the part of workmen was needed in self-defence. If, however, we turn to the action of the leaders of trades unions in the recent lamentable strikes, we are presented with a picture which many of us can only view as that of tyranny of the most close and oppressive kind, in which individual freedom cannot even be recognized. There are hundreds of owners of works who long to devote themselves to the true welfare of those they employ, but who can do little against the influence of the professional agitator, and are merely saddened by contact with prejudice and ignorance. I believe the view to be correct that some system by which the workman participates in the profits of enterprise will afford the most hope of putting an end to labour disputes, and we are told that profit-sharing tends to destroy the workmen's sense of social exclusion from the capitalistic board, and contents him by elevating him from the precarious position of a hired labourer. No pains should, therefore, be spared in perfecting a system of profit-sharing.

Pensions for long service are great aids to patience and fidelity, and very much may be hoped from the fact that strenuous efforts are being made by men really competent to lead. The Report of the Labour Commission which is now sitting will be looked for with keen interest. Watchful care over the health, interests, and instruction of the employed is exercised by many owners of works; and in this respect the Dowlais Works, which are being transplanted into your midst at Cardiff, have long presented a noteworthy example. Workmen must not forget that the choice of their own leaders is in their own hands, and on this the future mainly depends. "We may lay it down as a perpetual law that workmen's associations should be so organized and governed as to furnish the best and most suitable means for attaining what is aimed at—that is to say, for helping each individual member to better his condition to the utmost in body, mind, and property." These words will be found in the Encyclical Letter which Pope Leo XIII. has recently issued on the "Condition of Labour." To me it is specially interesting that the Bishop of Rome in his forcible appeal again and again cites the opinion of St. Thomas Aquinas, who was a learned chemist as well as a theologian.

Those of us who realize that "the higher mysteries of being, if penetrable at all by human intellect, require other weapons than those of calculation and experiment," should be fully sensible of our individual responsibility. Seeing that the study of the relations between capital and labour involves the consideration of the complex problems of existence, the solution of which is at present hidden from us, we shall feel with Andrew Lang that "where, as matter of science, we know nothing, we can only utter the message of our temperament." My own leads me to hope that the patriotism of the workmen will prevent them from driving our national industries from these shores; and I would ask those to whom the direction of the metallurgical works of this country is confided to remember that we have to deal both with metals and with men, and have reason to be grateful to all who extend the boundaries, not only of our knowledge, but also of our sympathy.

SECTION D.

BIOLOGY.

OPENING ADDRESS BY FRANCIS DARWIN, M.A., M.B., F.R.S., FELLOW OF CHRIST'S COLLEGE, CAMBRIDGE, PRESIDENT OF THE SECTION.

On Growth-curvatures in Plants.

A SEEDLING plant, such as a young sunflower, when growing in a state of nature, grows straight up towards the open sky, while its main root grows straight down towards the centre of the earth. When it is artificially displaced, for instance by laying the flower-pot on its side, both root and stem execute certain curvatures by which they reach the vertical once more. Curvatures such as these, whether executed in relation to light, gravitation, or other influences, may be grouped together as growth-curvatures, and it is with the history of our knowledge on this subject that I shall be occupied to-day. I shall principally deal with geotropic curvatures, or those executed in relation to gravitation, but the phenomena in question form a natural group, and it will be necessary to refer to heliotropism, and, indeed, to other growth-curvatures. The history of the subject divides into two branches, which it will be convenient to study separately.

When a displaced apogeotropic organ curves so as to become once more vertical, two distinct questions arise, which may be briefly expressed thus:—

(1) How does the plant recognize the vertical line; how does it know where the centre of the earth is?

(2) In what way are the curvatures which bring it into the vertical line executed?

The first is a question of irritability, the second of the mechanism of movement. Sachs has well pointed out that these two very different questions have been confused together (*Arbeiten*, ii. p. 282, 1879). They should be kept as distinct as the kindred questions, How, by what nervous apparatus, does an animal perceive changes in the external world; and how, by what muscular machinery, does it move in relation to such changes?

The history of our modern knowledge of geotropism may conveniently begin with Hofmeister's researches, because in an account of his work some of the points which re-occur in recent controversy are touched, and also because in studying his work the necessity of dividing the subject into the two above-named headings, Irritability and Mechanism, will be more clearly perceived.

In 1859 (*Berichte d. k. Sächs. Ges. d. Wiss.*), Hofmeister published his researches on the effect of disturbance, such as shaking or striking a turgescent shoot. This appears at first sight sufficiently remote from the study of geotropism, but the facts published in this work were the basis of the theory of geotropism formed by Hofmeister and accepted with some modification by Sachs. When an upright, vigorously-growing, turgescent shoot is struck at its base the upper end is made to curve violently towards the side from which the blow came. When the shoot comes to rest it is found to be no longer straight, but to have acquired a permanent bend towards the side on which it was struck. In explaining this phenomenon Hofmeister described those conditions of growth which give rise to what is known as the tension of tissues; these facts are still an important part of botanical study, though they hold quite a different position from that assigned to them by Hofmeister. The classification into active or erectile tissue and passively extended tissue was then first made. The pith, which is compressed, and strives to become longer, is the active or erectile part, the cortical and vascular constituents being passively extended by the active tissue. Hofmeister showed that when the shoot is violently bent the elasticity of the passive tissues on the convex side is injured by overstretching. The system must assume a new position of equilibrium; the passive tissues are now no longer equally resisting on the two sides, and the shoot must necessarily assume a curvature towards that side on which passive tissues are most resisting.

In a second paper, in 1860, Hofmeister (*Berichte d. k. Sächs. Ges. d. Wiss.*) applied these principles to the explanation of geotropism. It is true that in his second paper he does not refer to the former one, but I think that it can hardly be doubted that the knowledge which supplied the material for his paper of 1859 suggested the theory set forth in 1860. He had shown that in the system of tensions existing in a turgescent shoot lay the power of producing artificial curvatures, and he applied the same principle to the natural curvatures. When an apogeotropic organ is placed in a horizontal position, Hofmeister[1] supposed that the resisting tissues on the lower side became less resisting, so that they yielded more readily than those on the upper side to the longitudinal pressure of the turgescent pith. The system in such a case comes to rest in a new position, the shoot curving upwards; the passive tissues on the upper and lower sides once more resist the expansion of the pith in equal degrees. In this way Hofmeister hit on an explanation which, as far as mechanism is concerned, is in rough outline practically the same as certain modern theories, which will be discussed in the sequel.

His views resembled more modern theories in this, too: he clearly recognized that they were, *mutatis mutandis*, applicable to acellular[2] organs. The manner in which Hofmeister compared the mechanics of multicellular and acellular parts was curious; nowadays we compare the turgescent pith of a growing shoot with the hydrostatic pressure inside the acellular organ. Just as the pressure inside a single cell stretches the cell-walls, so in a growing shoot the turgescent pith stretches the cortex.

[1] Knight had previously suggested an explanation (Philosophical Transactions, 1806), which is so far similar, that the sinking downwards by gravitation of the juices of the plant is supposed to be the primary cause of apogeotropism. Knight's explanation of positive geotropism is practically the same as Hofmeister's.

[2] Sachs's term *acellular* is, in the present connection, equivalent to *unicellular*.

As pith is to cortex, so is cell-pressure to cell-membrane. But Hofmeister would not have accepted any such comparison. In the case of acellular organs he localized both the erectile and passive tissues in the membrane. The cuticle was said to be passively extended by the active growth of the inner layers of the cell-wall.

It is remarkable that the obvious source of power which the pressure of the cell-sap against the cell-walls supplies should have been so much neglected. This may perhaps be accounted for as a revulsion against the excessive prominence given to osmosis in the works of Dutrochet.

The great fault of Hofmeister's views was the purely mechanical manner in which he believed changes in extensibility in the passive tissues to be brought about. When an apogeotropic shoot is placed horizontal there would be a tendency, according to Hofmeister, for the resisting passive tissues along the lower side of the shoot to become waterlogged owing to the fluid in the shoot gravitating towards that side. They would thus be rendered more extensible, and the shoot would bend up, since its lower parts would yield to the erectile tissues in the centre. Such a conception excludes the idea of gravitation acting as a stimulus, and tends to keep geotropism out of the category in which it now takes its place along with such obvious cases of response to stimulation as the movements of Mimosa. In this respect it was a retrogression from the views of some earlier writers. Dutrochet's clear statement (1824) as to growth-curvatures being an affair of stimulus and response will be quoted lower down. Treviranu, in his "Physiologie" (1838), speaks of geotropism as a *Trieb*, or impulse, and adds that though there is no question of desire or sensation, as in the impulses of animals, yet geotropism must be thought of as something higher than a merely mechanical or chemical action.

In taking such a view Hofmeister naturally neglected the biological side of the study of geotropism. Now, we think of gravitation as a stimulus, which the plant translates according to its needs. The plant, so to speak, knows where the centre of the earth is, and either grows away from it, or towards it, according as either direction suits its mode of existence.

We have seen how Hofmeister's view enabled him to apply a common explanation to acellular and multicellular organisms. But it led him into an error which more than counterbalances the credit due to such a generalization—namely, into separating what are now universally considered parts of a single phenomenon—viz. negative and positive geotropism. He gave totally different explanations of the bending down of a root and the bending up a stem. It is well known that he supposed a root to be plastic, and to bend over by its own weight, like a tallow candle on a hot day or a piece of heated sealing-wax.

The development of a unified view of heliotropism, geotropism, and other similar curvatures is a part of my subject, and for that reason the curious want of unity in Hofmeister's views is interesting.

In 1865, Sachs published his "Experimental-Physiologie." He here accepts Hofmeister's views with certain modifications.

Irritability.

When by a touch on a trigger the explosion of a pistol is caused, we do not say that the pistol is irritable, but when in an organism a similar release of stored-up energy occurs, we apple the term *irritability* to the phenomenon, and we call the influency which produced the change a stimulus. At this time (1865) there was, as far as I can discover, no idea that growth-curvatures were produced by external influences acting as stimuli. Gravitation and light were supposed to act directly, and not as releasing forces. This is all the more remarkable, because Dutrochet[1] had expressed with great clearness the conception which we now hold. He wrote:—"La cause inconnue de l'attraction n'est que la cause occasionelle du mouvement descendant des racines et de l'ascension des tiges; elle n'en est point la cause immédiate; elle agit dans cette circonstance comme agent nervimoteur. Nous verrons plus bas de nouvelles preuves de la généralité de ce fait important en physiologie, savoir que les mouvements visibles des végétaux sont tous des mouvements *spontanés*, exécutés à l'occasion de l'influence d'un agent extérieur et non des mouvements imprimés par cet agent." Nothing could be more to the purpose than this, and it is one of the most curious points in the history of the subject that the

[1] "Recherches anat. sur la Structure intime, &c." (1824), p. 107. Dutrochet, however, was not consistent in this matter, and later on gave explanations as mechanical as Hofmeister's.

botanical mind should have taken more than fifty years to assimilate Dutrochet's view.

In 1868 Albert Bernhard Frank published his valuable "Beiträge zur Pflanzenphysiologie," which was of importance in more than one way. In this work the term "geotropism" was first suggested in imitation of the existing expression "heliotropism." This uniformity of nomenclature had an advantage beyond mere convenience, for it served to emphasize the view that the curvatures were allied in character. His criticisms of Hofmeister and Sachs were directed against the following views:—

(i.) That roots and other positively geotropic organs bend owing to plasticity. By repeating and varying certain older experiments, Frank helped materially to establish the now universally accepted view that positive geotropism is an active, not a passive, curvature, and that it depends, like apogeotropism, on unequal distribution of longitudinal growth. Here, again, he introduced unity, bringing what had been considered different phenomena under a common heading. By studying the distribution of growth and of tension in a variety of curvatures he helped still more to unite them under a common point of view.

(ii.) He showed that Hofmeister's classification of organs into those (1) which have and (2) which have not tension, was valueless in connection with growth-curvatures; that is to say, that apogeotropism is not necessarily connected with the form of longitudinal tension found in growing shoots, and that the distinct kind of tension existing in roots has no connection with their positive geotropism. His work thus served to bring the subject into a more purely physiological condition, not only by his downright opposition to a mechanical theory backed by the great name of Hofmeister, but also by giving importance to physiological individuality.

In 1870, Frank published a more important work, "Die natürliche wagerechte Richtung der Pflanzentheilen." This paper not only tended to unite geotropism and heliotropism by proving the phenomena described to be common to both categories, but it more especially widened the field of view by showing that horizontal growth must be considered as kindred to vertical growth, and thus introduced a new conception of the reaction of plants to light and gravitation which has been most fruitful.

Frank showed that certain parts of plants, for instance the runners of the strawberries, even when kept in the dark, grow horizontally, and when displaced from the horizontal returned to it. Here, said Frank, is a new type of geotropism, neither positive nor negative, but *transverse*. Ten years later Elfving (Sachs's *Arbeiten*, 1880), working in Sachs's laboratory, got similar results with rhizomes of Scirpus, &c. These experiments are more conclusive than Frank's in one way, because the strawberry runners when darkened were in abnormal conditions, whereas the rhizomes used by Elfving were normally freed from light-effects. When a rhizome which has been placed so as to point obliquely upwards, moves down towards the horizontal position it is, according to the old nomenclature, positively geotropic, and, *vice versâ*, when it reaches the horizontal from below it is negatively geotropic. But it cannot be both positively and negatively geotropic. We are bound to assume that it is so organized that it can only assume a position of rest, and continue to grow in a straight line when it is horizontal, just as an ordinary geotropic organ cannot devote itself to rectilinear growth unless it is vertical. In this way Frank's conception of transverse geotropism paved the way for the theory that there are a variety of different organizations (or, as we may now say, irritabilities) in growing plants; and that, whether a plant grows vertically upwards or downwards or horizontally, depends on the individual and highly sensitive constitution of the plant in question. It is, of course, true that those who seek for mechanical explanations of growth curvatures might be able to find such a one for transverse geotropism. But when Frank's conception has once been seized such views are less and less acceptable; and, judging from my own experience, I cannot doubt that Frank's work deserved to have a powerful effect in preparing the minds of physiologists for a just view of irritability.

The belief in transverse geotropism received interesting support from Vöchting's work ("Die Bewegung der Blüthen und Früchte," 1882) on the movement of certain flowers which retain a horizontal position under the influence of gravitation.

Frank's views, it may be added, were accepted by my father and myself in our "Power of Movement," in which the term diageotropism was proposed, and has been generally accepted, for transverse geotropism. Nevertheless, though Frank was undoubtedly right, his views were strongly opposed at the time. He held similar views on the effect of light, believing that the power possessed by leaves of placing themselves at right angles to the direction of incident light must be considered as a new type of heliotropic movement, transverse or diaheliotropism. Frank's views were criticized and opposed by De Vries (Sachs's *Arbeiten*, 1872), who, by means of experiments carried out in the Würzburg Laboratory, tried to show that Frank's results can be explained without having resort to new types of geo- or heliotropism. De Vries believed, for instance, that a leaf may be apheliotropic and apogeotropic, and that its horizontal position under vertical illumination is due to a balance struck between the opposing tendencies, one of which calls forth an upward, the other a downward curvature.

The same point of view occurs again in Sachs's paper on "Orthotrope and Plagiotrope Plant-members" (Sachs's *Arbeiten*, 1879). Sachs holds to the opinion that Frank's theory is untenable, that it is upset by De Vries, and that the oblique or horizontal position is to be explained as the result of a balance between opposing tendencies.

In a paper published the following year, 1880 (Journal Linn. Soc.), I attempted to decide between the opposing views. My experiments proved that at least certain leaves can place themselves at right angles to the direction of incident light when there is no possibility of a balance being struck. The outcome of my experiments was to convince me that Frank's views are correct—namely, that the quality of growth called transverse heliotropism does exist.

This view was accepted by my father in the "Power of Movement." The conclusions of Vöchting, in the *Bot. Zeitung*, 1888, and Krabbe in Pringsheim's *Jahrbücher*, 1889, vol. xx., are on the same side of the question.

The general result of these confirmations of Frank's conception has been to bring to the front a belief in the individuality of the plant in deciding what shall be the effect of external conditions. Such a view does not necessarily imply irritability in a strict sense, for Frank himself explained the facts, as we shall see, in a different way. But it could not fail to open our eyes to the fact that in growth-curvatures, as in other relations to environment, external changes are effective as guides or sign-posts, not as direct causes.

Frank saw clearly that plants may gain such various aptitudes for reacting to light and gravitation as best suit their modes of life.

In stating this view, he refers to the influence of the "Origin of Species," which had shown how any qualities useful to living things might be developed by natural selection. Frank described the qualities thus gained under the term *polarity*. He supposed that the cell-membranes of a transversely heliotropic leaf (for instance) were so endowed that a ray of light striking it obliquely from base to apex produced an increase of growth on the side away from the light; while a ray oblique from apex to base caused a reverse movement. The polarity-assumption of Frank is a purely gratuitous one, and, if strictly interpreted, hardly tends to bring growth-curvatures into harmony with what we know of the relation of life to environment.

It will no doubt appear to be a forcing of evidence if, after such a statement as the last, I still claim for Frank that he led the way to our modern view of irritability. I can, of course, only judge of the effect of his writings on myself, and I feel sure that they prepared me to accept the modern views. It must also be insisted that Frank, in spite of his assumption of polarity, seems to have looked at the phenomena in a manner not very different from ours of the present day. Thus, he compares the action of gravitation on plants to the influence of the perception of food on a chicken. He speaks, too, of custom (Journal Linn. Soc., 1880, p. 91), or use, building up the specialized "instinct" for certain curvatures. These are expressions consistent with our present views, and I think that Vines ("Physiology") is perfectly just in speaking of Frank's belief in different kinds of irritability, although in so judging he may perhaps have followed equity rather than law.

One of the chief bars to the development of our present views on irritability was the fact that simple growth in length is influenced, and markedly influenced, by differences in illumination. Plants grow more quickly, *cæteris paribus*, in darkness than in light. With this fact to go on, it was perfectly natural that simple mechanical explanations of heliotropism should be made. De Candolle, as is well known, explained such curvatures by the more rapid growth of the shaded side. Thus it

came about that heliotropism was discussed, for instance, in Sachs's "Text-book," edit. 4, 1874, under the same heading as the influence of light on rectilinear growth.

Shortly afterwards, in 1876, a pupil of Sachs—Müller-Thurgau—published ("Flora") a research carried out in the Würzburg Laboratory, which is of some importance. In the introductory remarks he wrote:—"It has been hitherto supposed that heliotropic curvatures depend on a difference in intensity of illumination on the two sides. Sachs came to a different opinion in his work on geotropism: he found himself compelled to believe that in heliotropic, just as in geotropic curvatures, it is not a question of different intensities on opposite sides, but rather that heliotropic effect depends on the direction of the light."[1]

Müller's research gave weight to this union of geo- and heliotropic effects by showing a number of resemblances in the manner and form of the two curvatures. Again, when it was found[2] that apheliotropic organs are influenced by light and darkness in precisely the same manner as positively heliotropic ones, it became clear that the mechanical explanation of De Candolle was untenable for negatively heliotropic organs, it might still no doubt be upheld for positively heliotropic organs, but, as a matter of fact, it was not so upheld. There was a tendency to unify our view of growth-curvatures, and the union of the two forms of heliotropism gave strength to the movement. Nor was this all; when it became clear that light did not produce heliotropic curvatures by direct mechanical effect, it was natural to remember that gravitation has none either; we cannot point to any reason (except the crudest ones) why the lower side of a horizontal stem, or the upper side of a horizontal root, should grow the faster for the direct effects of gravitation. That being so, light and gravitation could be classed together as external agencies acting, not directly, but in some unknown indirect manner. I do not imply that such a result followed immediately, but that the line of research above alluded to helped in some degree to lead the way to a belief in growth-curvatures as phenomena of irritability.

When my father was writing our book, "The Power of Movement in Plants" (1880), in which he adopted to the fullest extent a belief that growth-curvatures are phenomena of irritability, the only modern statement of such a view which he could find was in a passage by Sachs (*Arbeiten*, ii., 1879, p. 282), where he writes that "The living material of plants is internally differentiated in such a way that different parts are supplied with specific energies resembling those of the sensory-nerves (*Sinnesnerven*) of animals. Anisotropy in plants fulfils the same purpose as do sense-perceptions in animals."

The idea of irritability as applied to growth-curvatures is expressed with sufficient clearness in "The Power of Movement." Thus, for the case of geotropism we wrote (p. 521):—"Different parts or organs on the same plant, and the same part in different species, are thus excited to act in a widely different manner. We can see no reason why the attraction of gravity should directly modify the state of turgescence and subsequent growth of one part on the upper side, and of another part on the lower side. We are therefore led to infer that both geotropic, apogeotropic, and diageotropic movements, the purpose of which we can generally understand, have been acquired for the advantage of the plant by the modification of the ever-present movement of circumnutation. This, however, implies that gravitation produces some effect on the young tissues sufficient to serve as a guide to the plant." A similar view is given for heliotropism. It should be noted that the essence of the view—namely, that light and gravitation act as guides or landmarks by which the plant can direct itself—can be held without a belief in circumnutation.

In Pfeffer's admirable "Pflanzenphysiologie," 1881, the conception of stimulus and reaction is fully given, and is applied, among other cases, to that of heliotropism and geotropism. Pfeffer states clearly, and without reserve or obscurity, the view that light and gravitation act as stimuli or releasing forces, in manners decided by the organization of the plant. Pfeffer seems to me to be the first writer who has treated the subject fully and consistently.

In Sachs's "Vorlesungen" (1882), a view similar to that briefly sketched in his paper of 1879 is upheld. Geotropism and heliotropism are described as *Reizerscheinungen*, *i.e.* phenomena of stimulation. The phenomena in question are described under the heading Anisotropy, a word which expresses, according to Sachs (p. 855), "the fact that different organs of a plant under the influence of the same external forces assume the most varied directions of growth." In another passage (p. 859) he states that the anisotropy of the different organs "is nothing else than the expression of their different irritability to the influence of gravity [and] light, &c."

Vines ("Physiology of Plants"), who has recently (1886) summarized the evidence on growth-curvatures, and whose researches on kindred subjects entitle his opinion to respect, accepts fully the view that gravitation, light, &c., act as stimuli.

It is not necessary to trace the subject further, the views under discussion being now well-recognized canons of vegetable physiology.

I cannot, however, omit to mention Pfeffer's (*Tübingen. Untersuchungen*, vol. i.) brilliant researches on the chemotaxis (irritability to certain reagents) of low organisms, such as antherozoids and bacteria. To take a single instance, Pfeffer showed that the antherozoids, in responding to the effect of malic acid, follow precisely the same law that in animals correlates the strength of stimulus and amount of effect. This result, although it has no direct connection with growth-curvatures, is nevertheless of the highest importance in connection with the general question of vegetable irritability.

Nor can I omit to mention the ingenious reasoning by which Noll (Sachs's *Arbeiten*, vol. ii. p. 466) localized the seat of irritability in a vegetable cell. He points out how in acellular plants, such as Caulerpa or Derbesia, the flowing protoplasm may travel from positively geotropic root to apogeotropic stem, and he argues from this that the motile endoplasm cannot be the seat of specific irritability. The flowing plasma, which is always changing its position with regard to external forces, must be as fully incapacitated from responding to them as though the plant were turning on a klinostat. It follows from this that it must be the stationary ectoplasm which perceives external change. From a different point of view, this is what we should expect—we should naturally suppose that the part which regulates the growth of the membrane, and therefore the curvature of the cell, should be the irritable constituent of the cell contents.

In attempting to trace the history of the establishment of growth-curvatures as phenomena of irritability, I have been forced to confine myself to a slight sketch. I have found it impossible to give a full account of the course of research on the subject. I have given an account of some of the halting-places in the journey of thought, but not to the manner in which belief has travelled from stage to stage. Far greater knowledge than mine would be required to compile such an itinerary.

Mechanism.

The first step in advance of Hofmeister's views was the establishment that the curvatures under consideration are due to unequal growth—that is to say, to an excess of longitudinal growth on the convex than the concave side. It is not, however, easy to say how far Hofmeister had this idea, for it, in fact, depends on how we define "growth." Hofmeister knew, of course, that the convex side of a curved shoot was longer than it had been before the curvature occurred; this is a mathematical necessity. But he also made out the important point that the concave side increases in length during the curvature. These permanent elongations he must have known to be growth, but his attention was directed to what is, after all, the more important point—namely, *why* it was that unequal elongation took place.

Sachs, in his "Experimental-Physiologie," held that growth-curvatures are due to unequal growth. In his "Text-book" (1874), English translation, 1882, p. 853, the author, referring to Hofmeister's work, says:—"I pointed out that the growth of the under surface of an organ capable of curving upwards was accelerated, and that of the upper surface retarded; I did not at the time express an opinion as to whether these modifications of growth were due to an altered distribution of plastic material or to a change in the extensibility of the passive layers of tissue." Frank's already-quoted paper made valuable contributions to the subject. He showed that the epidermic cells on the convex side of the root are longer than those on the concave side—that is, they have grown more; he explained apogeotropic curvatures in precisely the same way. He showed, moreover, that the

[1] In his "Vorlesungen," p. 854, Sachs states that he wrote Müller-Thurgau's introduction.

[2] Schmitz, *Linnæa*, 1843; Müller-Thurgau ("Flora," 1876); F. Darwin, Sachs's *Arbeiten*, 1880. The two latter researches were carried out under the direction of Sachs in his laboratory.

sharp curve close to the tip of a geotropic root, and the long gradual curve of an apogeotropic shoot, are necessary consequences from the manner in which growth is distributed in these parts. He demonstrated that rectilinear growth and geotropic curvature require the same external conditions; that, for instance, a temperature low enough to check growth also puts a stop to geotropism.

The distribution of longitudinal growth, which produces geotropism, was afterwards studied by Sachs (*Arbeiten*, i. p. 193, June 1871), who thoroughly established the fact that the convex side grows faster, while the concave side grows slower, than if the organ had remained vertical and uncurved.

These facts are of interest in themselves, but they do not, any more than Frank's results, touch the root of the matter. Until we know something of the mechanics of rectilinear growth, we cannot expect to understand curves produced by growth. The next advance in our knowledge did, in fact, accompany advancing knowledge of rectilinear growth. It began to be established, through Sachs's work, that turgescence is a necessary condition of growth. A turgescent cell is one which is, as it were, over-filled with cell sap; its cell-walls are stretched by the hydrostatic pressure existing within. In osmosis, which gives the force by which the cells are stretched, a force was at hand by which growth could be conceived to be caused. The first clear definition of turgor, and a statement of its importance for growth, occurs in Sachs's classical paper on growth (*Arbeiten*, p. 104, August 1871).

As soon as the importance of turgor in relation to growth was clearly put forward, it was natural that its equal importance with regard to growth-curvatures should come to the fore, and that increased growth on the convex side (leading to curvature) should be put down to increased internal cell-pressure in those tissues. In the fourth edition of Sachs's "Lehrbuch" (1874), Eng. trans., 1882, p. 834, such a view is tentatively given, but the author saw very clearly that much more evidence was needed before anything like a conclusion as to the mechanism of movement could be arrived at. The difficulty which faced him was not a new one—in a slightly different form it had occurred to Hofmeister—the question, namely, whether the curvatures of acellular and multicellular organs depend on the same or on different causes. If one explanation is applicable to both, then we must give up as a primary cause any changes in the osmotic force of the cells. For no change in the pressure inside a cell will produce a curvature in that cell, whereas, in a multicellular organ, if in the cells in one longitudinal half an increase of osmotic substances takes place, so that the cell-walls are subject to greater stretching force, curvature will take place.

On the other hand, if the cause of bending of acellular and multicellular organs is the same, we must believe that the curvature takes its origin in changes in the cell-walls. In an acellular organ, if the cell-membranes yield symmetrically to internal pressure, growth will be in a straight line; if it yields asymmetrically it will curve. Thus, if the membrane along one side of a cell becomes more or less resisting than the rest of the membrane, a curvature will result.

If we are to apply strictly the same principle to acellular and multicellular organs, we must suppose that the whole organ curves, because each individual cell behaves like one of the above-described free cells, the curvature of the whole resulting from the sum of the curves of the separate cells. This was Frank's view, and it also occurs in Sachs's "Text-book" (1874), Eng. trans., 1882, p. 842.

Are we bound to believe that the mechanism of acellular and multicellular curvatures is so strictly identical as Frank supposed? In the first place, it is not clear why there should be identity of mechanism in the movements of organs or plants of completely different types of structure. The upholders of the identity chiefly confine themselves to asseveration that a common explanation must apply to both cases. I believe that light may be thrown on the matter by considering turgescence, not in relation to growth, but in regard to stability of structure.

An acellular organ, such as the stalk of the sporangium of Mucor, owes its strength and stiffness to the tension between the cell contents and the elastic cell-wall, but it does not follow from this that in multicellular organs strength and stiffness are due to the sum of the strength of its individual cells. Indeed, we know that it is not so: the strength of a multicellular organ depends on the tension between pith and cortex. It is, in fact, a model of the single cell; the pith represents the cell-sap, the cortex the cell-wall. Here, then, it is clear that the function performed by the cell-wall in one case is carried out by cortical tissues in the other. If this is the case for one function, there is no reason why it should not hold good in another, viz. the machinery of movement.

If we hold this view that the cortex in one case is analogous with a simple membrane in the other, we shall not translate the unity of acellular and multicellular organs so strictly as did Frank. Indeed, we may fairly consider it harmonious with our knowledge in other departments to find similar functions performed by morphologically different parts. The cortex of a geotropic shoot would thus be analogous with the membrane of a geotropic cell in regard to movement, just as we know that these parts are analogous in regard to stability.

In spite of the difficulties sketched above, one writer of the first rank, namely, H. de Vries, has upheld the view that growth-curvatures in multicellular organs (*Bot. Zeitung*, 1879, p. 835), are due to increased cell-pressure on the convex side; the rise in hydrostatic pressure being put down to increase of osmotic substances in the cell-sap of the tissues in question. Such a theory flowed naturally from De Vries's interesting plasmolytic work (*ibid.* 1877, p. 1). He had shown that those sections of a turgescent shoot which were in most rapid growth show the greatest amount of shortening when turgescence is removed by plasmolysis. This was supposed to show that growth is proportional to the stretching or elongation of the cell-walls by turgor. Growth, according to this view, consists of two processes: (1) of a temporary elongation due to turgescence, and (2) of a fixing process by which the elongation is rendered permanent. De Vries assumed that where the elongation occurred, its amount must be proportional to the osmotic activity of the cell contents; thus neglecting the other factor in the problem—namely, the variability in the resistance of the membranes. He applied the plasmolytic method to growth-curvatures, and made the same deductions. He found that a curved organ shows a flatter curve [1] after being plasmolyzed. This, according to his previous argument, shows that the cell-sap on the convex is more powerfully osmotic than that on the concave side. This again leads to increased cell-stretching, and finally to increased growth.

The most serious objection to De Vries's views is that the convex half of a curving organ does *not* contain a greater amount of osmotically active substance.[2] It must, however, be noted in the heliotropic and geotropic curvature of pulvini, there is an osmotic difference between the two halves[3]—so that, if the argument from uniformity is used against De Vries (in the matter of acellular and multicellular organs), it may fairly be used in his favour as regards the comparison of curvatures produced with and without pulvini.

It is not easy to determine the extent to which De Vries's views on the mechanics of growth-curvature were accepted. The point, however, is of no great importance, for the current of conviction soon began to run in an opposite direction.[4]

Sachs ("Lehrbuch," ed. 4, Eng. trans. p. 835) had already pointed out that attention should be directed to changes in extensibility of cell-walls as an important factor in the problem.

Wiesner, in his "Heliotropische Erscheinungen" (*Wiener Sitzungsb.*, vol. lxxxi., 1880, p. 7; also in the *Denkschriften*, 1882), held that the curvature of multicellular organs is due both to an increase of osmotic force on the convex side, and to increased ductility[5] of the membranes of the same part. He repeated De Vries's plasmolytic experiments, and made out the curious fact that in many cases the curvature is increased instead of being diminished. He attributed the result to the concave tissues being more perfectly elastic than ductile convex tissues, so that when turgescence is removed, the more elastic tissues shorten most, and, by diminishing the length of the concave side, increase the curvature.

Strasburger, in his "Zellhäute" (1882), suggested that growth-curvatures are due to increased ductility of the convex membranes, and gave a number of instances to prove that a change to a ductile condition does occur in other physiological processes, such as the stretching of the cellulose ring in Œdogonium to a

[1] Frank made similar experiments, but failed to find any diminution of curvature.

[2] Kraus, *Abhand. Nat. Gesell zu Halle*, xv., 1882. See also a different proof by Wortmann, *Deutsch Bot. Gesell.*, 1887, p. 459.

[3] Hilburg in Pfeffer's *Tübingen. Untersuch.*, vol. i., 1881, p. 31.

[4] An opportunity will occur later on for referring to some details of De Vries's work not yet noticed.

[5] Weinzierl, *Sitzungsb Wien.*, 1877, showed that strips of epidermis taken off the convex side of heliotropically curved flower-stalks of tulip and hyacinth were about twice as extensible when stretched by a small weight, 7·5 grammes, as approximately corresponding strips for the concave side.

uniform thin membrane, the branching of Cladophora, and the escape of sexual products in certain Algæ.

We now pass on to the work of two observers, Wortmann and Noll, who have devoted special attention to mechanism of curvatures. Wortmann (*Bot. Zeit.*, 1887, p. 785) started on the assumption, already several times mentioned, that the growth-curvature of acellular and multicellular organs must have a common cause. He began by testing Kohl's statement (*Bot. Hefte*, Marburg, Heft v. [I have not seen Kohl's paper]) that when the sporangiferous hypha of a Phycomyces curves apogeotropically or heliotropically, &c., there is a collection of protoplasm on the concave wall. Wortmann principally investigated the curvature discovered in Phycomyces by Errera (*Bot. Zeitung*, 1884) which can be produced by contact. When the hypha is touched with a glass filament or with a platinum wire, or by allowing a speck of indian ink to dry on it, it curves over towards the touched side. The hypha is so highly sensitive to contact that it curves in from three to six minutes; it is clearly a growth-curvature, for it only occurs in the part of the hypha which is growing. In curvatures thus produced, as well as in apogeotropic and heliotropic curvatures, the accumulation of protoplasm on the concave side is, according to Wortmann, clearly visible, and, what is more important, the membrane becomes thicker on the concave side, sometimes twice as thick as on the opposite side of the cell. In consequence of the unequal thickening of the membranes, the cell is supposed to yield asymmetrically cell-pressure, and the necessary consequence is that the cell grows into a curved form.

In applying the same method of investigation to multicellular parts, Wortmann followed Ciesielski (Cohn's "Beiträge," 1872, p. 1), who noticed that in geotropically curved roots the cells of the concave (lower) side of the organ are much more densely filled with protoplasm than are the convex cells. Sachs ("Vorlesungen," p. 842) describes a similar state of things in the halms of grasses, and Kohl, again, in tendrils and the stems of climbing plants.

Wortmann first of all made sure that no redistribution of protoplasm could be observed in the *individual* cells of curving multicellular organs. If each cell behaved independently like a free cell, we might expect to find a collection of protoplasm on the concave wall of all the constituent cells of a curving shoot. But this is not the case. Nor at first could any microscopic differences be made out between the concave and convex tissues of a curving shoot. But when the stimulus was made to act for a long time, differences were apparent. A young Phaseolus plant was placed so that the epicotyl was horizontal and was forced to grow in the horizontal direction by a thread attached to the end of the stem, passing over a pulley and fastened to a weight. Here the geotropic stimulus could continue to act for 24–36 hours, and under such conditions a marked change in the tissues was visible. The cells of the cortex on the upper side became densely filled with protoplasm, while the lower cortical cells were relatively poor in protoplasmic contents. The same changes in the membranes occur as those noticed in Phycomyces—that is to say, the walls of the cortex on the upper side are very much thicker than those on the lower side.[1]

Since the walls of the cortical cells have become more resisting on the upper than on the lower side, then (assuming the osmotic expanding force to be the same in both cases) the growth will be quicker on the lower side, and the shoot will curve upwards. Wortmann states that his observations account for the fact that the convex side grows quicker, not merely than the concave, but than a normal unbent shoot. But he does not seem to have compared the thickness of the convex cell-walls with the normal, although he states that they are poorer in protoplasm than is usual, and from this it may, according to his views, be perhaps assumed that the membranes are abnormally thin.

Wortmann points out that his views account for two well-known features in growth-curvatures, viz. the *latent period* and the *after-effect*. If a curvature can only occur when a difference in structure of cell-walls has arisen, it is certainly natural that some time should occur before the curvature is apparent. I do not lay much stress on this part of the subject, as I feel sure the whole question of latent period needs further investigation. With regard to after-effect it is true that Wortmann's views account for the continuance of curvature after the stimulus has ceased to act.

Wortmann attaches great importance to another point in his theory, which, could it be established, would be of the greatest interest, and would unite under a common point of view, not only acellular and multicellular organs, but also naked protoplasm, *e.g.* the plasmodia of myxomycetes. The view in question was tentatively suggested by Sachs ("Lehrbuch," 1874; Eng. trans., 1882, p. 841), and mentioned by Pfeffer ("Pflanzenphysiologie," ii. p. 331) in a similar spirit. The apogeotropic curvature of a Phycomyces-hypha is supposed to be due to the unequal thickening of the membrane on the upper and lower sides, and this to be due to the migration of protoplasm from the lower to the upper side of the cell. In the same way in a multicellular organ the protoplasm is supposed to migrate from the lower cortex and pith to the upper cortex and pith, such migration being rendered possible by the now generally admitted intercellular protoplasmic communication. Thus the apogeotropism of a cell or a multicellular part would be due to the apogeotropism or tendency to migrate vertically upwards of the protoplasm. There are great difficulties in the way of accepting this attractive theory.

Noll (Sachs's *Arbeiten*, 1888, p. 530) states that when a curved Phycomyces-hypha, in which protoplasm has accumulated in the upper (concave) side, is reversed so that the mass of protoplasm is below, it does not migrate upward again, as might be expected. Moreover, he points out that in Nitella and in Bryopsis the circulating protoplasm continues in movement, and does not accumulate in any part of the cell. Lastly, there seems, as Noll points out, a difficulty in believing in the migration of protoplasm through the very minute pores by which the plasma strands pass from cell to cell. There seems much probability in Noll's view that the plasmic strands only serve for the passage of impulses, or molecular changes, and that they consist of ectoplasm alone, not of the endoplasm which Wortmann describes as the migratory constituent of the cell.

Wortmann's theory has been criticized by Elfving (*Finska Vet. Soc. Förhand.*, Helsingfors, Bd. xxx., 1888). The essence of Elfving's paper is that appearances similar to those described by Wortmann can be produced by curvatures not due to stimulation. Thus, when Phycomyces is made to grow against a glass plate it is mechanically forced to bend. Yet here, where there is no question of stimulation, the plasma collects along the concave side of the cell. Elfving concludes that the visible changes are the result and not the cause of the curvature. Elfving also produced curvature in Phaseolus by bending the apex of the plant towards its base and tying in that position. Under these conditions the convex side of the shoot showed the changes described by Wortmann in geotropic plants. Here again Elfving gives reason to believe that the thickening of the cell-walls is a result, not of curvature, but of strain mechanically produced. When a plant is prevented from executing an apogeotropic movement it is clear that a longitudinal strain is put on the upper (concave) side. But the longitudinal strain in Elfving's plants is on the convex side. Therefore, if, as Elfving believes, the visible changes are due to strain, they should, as they do, occur on the convex side in his experiments, on the concave in Wortmann's.

Wortmann replied in the *Bot. Zeitung*, 1888, p. 469, and attempted to explain how Elfving's results might be explained and yet his own theory hold good. The reply is by no means so strong as the criticism, and it must be allowed that Elfving has seriously shaken Wortmann's argument.

Somewhat similar criticisms have been made by Noll (Sachs's *Arbeiten*, 1888, p. 496). In the acellular plants, Derbesia and Bryopsis, Noll studied growth-curvatures, and was quite unable to detect any thickening of the concave cell-walls, except when the curvatures were very sudden, and in these cases the result could equally well be produced by mechanical bending.

Noll further points out what is undoubtedly a fault in Wortmann's theory—namely, that he explains the retardation on the concave rather than acceleration on the convex side. This criticism is only partially just, for though Wortmann's description only shows a *relative* thinness of the walls on the convex side, yet it is clear he believed there to be an absolute diminution of resisting power on that side.

Noll's experiments with grass halms show clearly that acceleration of growth on the convex side is the primary change, rather than retardation along the concave half. When the halms are fixed in horizontal glass tubes, so that they are stimulated but unable to bend, the lower half of the pulvinus forms an irregular out-growth, increasing radially since it is not able to increase longitudinally.

[1] Both protoplasmic change and thickening of cell-walls occur to some extent in the pith.

A similar argument may be drawn from Elfving's experiments. He found that the pulvini of grass halms placed on the klinostat increase in length. This experiment shows incidentally that the klinostat does not remove but merely distribute equally the geotropic stimulus: also that geotropic stimulus leads to increased, not to diminished growth. The same thing is proved by the simple fact that a grass halm shows no growth in its pulvinus while it is vertical, so that when curvature begins (on its being placed horizontal) it must be due to acceleration on the convex, since there is no growth on the concave side in which retardation could occur. Noll's view is that the primary change is an increase in extensibility of the tissues on the convex side. This view he proceeded to test experimentally. A growing shoot was fixed in a vertical position, and a certain bending force was applied to make it curve out of the vertical, first to the right and then to the left. If the cortical tissues are, at the beginning of the experiment, equally resisting all round, it is clear that the excursions from the vertical to the right and left will be equal. As a matter of fact the excursions to the right and left were nearly the same, and the difference was applied as a correction to the subsequent result. The shoot was then placed horizontally until geotropic or other curvature was just beginning, when the above bending experiment was repeated. It was then found that when it was bent so that the lower side was made convex, the excursion was greater than it had been. In the few experiments given by Noll the excursion in the opposite direction (stretching of the concave side) was less than it had been, and he states that all the other experiments showed a similar result. The increased extensibility of the convex side is clearly the most striking part of the phenomenon, but I fail to see why Noll takes so little notice of the diminution in the extensibility of the concave side, which is only mentioned towards the end of his paper (*loc. cit.* p. 529). Yet such a diminution is a necessary factor in the mechanism of curvature. It should be noted that results like Noll's might be obtained under other conditions of growth curvatures. Thus if De Vries's view were the true one, and the curvature were due to difference in osmotic force on the convex and concave sides, the shoot would react differently in the two directions; for instance, the concave side would be the more easily compressed. Noll and Wortmann's explanations differ in this: the former lays the greater stress on the increased extensibility of the convex side, the latter on the diminution of that of the concave side. Again, Wortmann explains the difference in extensibility as due to differences in thickness of the cell-walls. Noll gives no mechanical explanation, but assumes that the ectoplasm has the power of producing changes in the quality of the cell-wall in some unknown way.

In the early stages of curvature, a phenomenon takes place to which Noll attaches great importance as supporting his view. When a curved organ is plasmolyzed, it suffers a diminution of curvature, as De Vries showed, but Noll[1] has proved that in the early stages of curvature a contrary movement occurs—that is to say, the curvature is increased. This seems to show that the yielding of the convex side is owing to a ductility, which prevents its holding its own against the more perfect elasticity of the concave side. But this is only the beginning of the phenomenon; as the plasmolyzing agent continues to act, a reverse movement takes place, the well-known flattening of the curvature described by De Vries. It is to me incomprehensible how in a given condition of cell-walls these results can occur in different stages of plasmolysis. I can understand one occurring when the curvature is recent, and the other, the flattening of the curve, occurring when the ductile convex parts have reacquired elasticity. The fact undoubtedly is as Noll describes it: his explanation seems to me inadequate.

We have now seen that the most acceptable theory of the machinery of these curvatures is in its main features akin to Hofmeister's, the power of elongation supplying the motive force, while the varying extensibility of the membranes determines the nature and direction of the bend.

The question now arises: Is it possible by these means to account for all the facts that must be explained? Taking the theory for which there is most to be said on experimental grounds—viz. Noll's—it will be noted that it is essentially connected with the doctrine of growth by apposition. The question, therefore, whether the apposition-theory is sufficient to account for the phenomena of ordinary growth, may be applied *mutatis mutandis* to growth-curvature. This doctrine in its original purity absolutely requires turgescence to account for the elongation of growth. The older layers, separated from the ectoplasm by the younger layers of cell-wall, can *only* be elongated by traction. Growth by intussusception does not absolutely require this force; the theory that the micellæ are separated by traction, and thus allow intercalation of fresh micellæ, is a view for which Sachs is chiefly responsible.

Since surface-growth by apposition is absolutely dependent on the traction exercised by cell-pressure, it is a fair question—how far growth is influenced by forcible elongation. Baranetzky (*Mem. Acad. St. Pét.*, v. vol xxvii. p. 20) states that when a plant is subject to traction, as by even a small weight attached to the free end, the rate of growth is lowered. Ambronn (Pringsheim's *Jahrb.*, xii.), as Zimmermann points out in the same connection, found no increased elongation of collenchyma when stretched for some days by means of a weight. A greater difficulty is that growth may be absolutely and at once stopped by placing the growing organ in an atmosphere free from oxygen (Wieler, Pfeffer's *Untersuch.*, Bd. i. p. 189). Such treatment apparently does not diminish turgescence, yet growth stops. If the cell-walls are increasing in length by mechanical stretching, and if the turgor is not interfered with, increase in length ought to continue. The same thing applies to curvatures. Wortmann has shown (*Bot. Zeit.*, 1884, p. 705) that in an atmosphere of pure hydrogen a geotropic curvature which has begun in ordinary air cannot continue; in other words, after-effect ceases. This seems to me inexplicable on Noll's or Wortmann's theories; the convex side has become more extensible than the concave, turgescence, as far as we know, continues, yet no after-effect is observed. The same result may be gathered from Askenasy's[1] interesting experiments on the growth of roots. He showed that lowering the temperature has an almost instantaneous inhibitive effect on growth. Thus maize roots (at a temperature of 26°·6) growing at the rate of 33 divisions of the micrometer per hour, were placed in water at 5°, and absolutely no growth occurred during the following ten minutes, in which the thermometer rose to 6°·5. This result is all the more valuable because we know from Askenasy's[2] other results that the turgor, as estimated by plasmolytic shortening, is about the same whether the root is in full growth or not growing at all. This is not conclusive, for if the growing cell-walls were ductile they might shorten but little although under great pressure, whereas the non-growing cells might shorten a good deal, owing to their more perfect elasticity;[3] therefore Askenasy's plasmolytic results are not in this particular connection of great importance, except as showing that the non-growing roots were certainly to some extent turgescent.

There are other facts which make it extremely difficult to understand how surface-growth can depend on cell-pressure. Nägeli ("Stärkekörner," p. 279) pointed out that the growth of cylindrical cells which elongate enormously without bulging outwards laterally, is not explicable by simple internal pressure. An internodal cell of Nitella increases to 2000 times its original length, while it only becomes ten times as wide as it was at first. The filaments of Spirogyra become very long, and keep their original width. Nägeli found that in Spirogyra the shortening produced by plasmolysis was practically the same in the longitudinal and in the transverse direction. He therefore concluded that the growth of Spirogyra cannot be accounted for by the cell-wall being differently extensible along different axes. But it must once more be pointed out that this type of plasmolytic experiment has not the force which Nägeli ascribes to it. If the cell-wall stretched like putty in one direction and like india-rubber in the other, there might be no plasmolytic shortening in the line of greatest growth. Nevertheless, in spite of this flaw in Nägeli's argument, great elongation in a single direction remains a problem for those who believe in surface-growth by apposition.

The point of special interest is that differences in extensibility in different directions cannot be supposed to exist in a homogeneous membrane. If *any* purely physical characters can explain the facts, they must be architectural characters. That is to say, we must be able to appeal to remarkable structural differences along different axes if we are to explain the facts.

[1] The similar results obtained by Wiesner are noticed above.

[1] *Deutsch. Bot. Ges.*, 1890, p. 61. This paper contains an excellent discussion on the mechanics of growth, to which I am much indebted.

[2] *Loc. cit.* p. 71.

[3] Wiesner (*Sitz. Wien. Akad.*, 1884, vol. lxxxix.-xc., Abth. i. p. 223) showed that under certain conditions decapitated roots grow much more quickly than normal ones, yet the amount of plasmolytic shortening is less. Decapitated: growth 79 per cent.; plasmolytic shortening, 8 per cent.; normal: growth, 39 per cent.; shortening, 13 per cent.

Such structural differences do, of course, exist, but whether they are sufficient to account for the phenomena is a different question. Strasburger ("Zellhäute," p. 194) supposes that the elasticity of a cell-wall depends on the last-formed layers, and as in these the microsomes are seen arranging themselves in lines or patterns, we have a heterogeneity of structure which may or may not be sufficient.

We have now seen that it is difficult to believe, although it is not inconceivable, that the extending force of cell-turgor, combined with differences in extensibility of the membranes (depending on structural characters), may account for the phenomena of rectilinear growth. But, even if we allow that this is so, how are we to apply the same explanation to growth-curvatures? How are we to account for the rapid changes in extensibility necessary to produce geotropic or heliotropic curvatures? The influences which Strasburger and Noll suppose to act on the cell-walls and render them ductile cannot account for extensibility in one direction only. Nor does Wortmann's theory, that difference in extensibility depends on difference in thickness, meet the case completely. What we need is an increase in longitudinal, not in general extensibility. I presume that these writers might say that the excess in longitudinal extensibility is always present whether general extensibility is greater or less. In the meanwhile we must pass on to more recent researches on surface-growth by apposition.

In Strasburger's later work ("Histologische Beiträge," 1889), his views on growth have undergone considerable modification. The study of certain epidermic cells, of the folds in membranes, and the repetition of Krabbe's work on certain bast fibres, have convinced him that apposition does not account for all forms of growth. Krabbe (Pringsheim's *Jahrb.*, xviii.) showed that in full-grown sclerenchyma (*e.g.* in Oleander) local widenings occur without any such amount of thinning in the membrane as would occur if the bulging were due to stretching. The only possible explanation seems to be that there is a migration of new material into the cell-wall. Such intussusception might be, as Nägeli supposed, a flow of fluid out of which new micellæ crystallize; but it is now established that cellulose arises as a modification of protoplasm, so that it would harmonize with our knowledge of the origin of cellulose if we assume that intussusception was preceded by a wandering of protoplasm into the cell-wall. Such a state of things would render possible the regulation of longitudinal growth in the case of Nitella and Spirogyra, already alluded to, as well as in growth-curvatures. This view might also harmonize with Wiesner's theory (*Sitz. Wien. Akad.*, 1886, vol. xciii. p. 17) that the cell-wall contains protoplasm as long as it continues to grow.

For the sake of brevity I content myself with the above examples: I think it will be allowed that there is a focussing of speculation from many sides in favour of "active" surface-growth—or, what is perhaps a better way of putting it, in favour of a belief that the extension of cell membranes depends on physiological rather than physical properties, that it is in some way under the immediate control of the protoplasm. We may take our choice between Wiesner's wall-protoplasm (dermatoplasm), protoplasmic intussusception as conceived by Strasburger, or the action of the ectoplasm in the manner suggested by Vines,[1] who supposes that the crucial point is a change in the motility of the protoplasm, not of the cell membrane. The latter theory would undoubtedly meet the difficulties—if we could believe that so yielding a substance as protoplasm could resist the force of turgor.

The great difficulty is, as it seems to me, that since, *e.g.* in Caulerpa, surface-growth is clearly due to stretching, as Noll has demonstrated, and since in osmotic cell-pressure a stretching force does exist, it cannot be doubted that turgor, and ordinary physical extensibility are conditions of the problem. This remains true in spite of Klebs's (*Tübingen. Untersuchungen*, ii. p. 489) curious observations on the growth of plasmolyzed Algæ, or in spite of the fact that pollen tubes may grow without turgor, in spite of the same being perhaps true of young cells filled with protoplasm (see Noll, *Würzburg. Arbeiten*, iii. p. 530). In the face of all these facts, osmotic pressure in the cell must remain a *vera causa* tending to surface-growth.

If we accept some form of "active" surface-growth, we must deal with turgor in another way, although to do so may require a violent exercise of the imagination. Are we to believe, for instance, that the function of turgescence is the attaining of mechanical strength? If we hold that cell-walls increase in area independently of turgor, we shall be forced to invent a hypothesis such as the following—which I am far from intending to uphold. It is possible to imagine that the function of the force of turgor is merely to spread out the growing membrane to its full extent, and, as it were, to make the most of it. Turgor would in this respect play the part occupied by the frame used in embroidery, making it easier to carry on the work satisfactorily, but not being absolutely necessary. When mechanical strength is gained by turgor (as in Mucor), instead of by brute strength of material, as in a tree-trunk, a great economy in cellulose is effected. If turgor played our hypothetical part of smoothing out the membrane and insuring that it shall occupy as large a space as possible, it would effect the same kind of economy.

It is not necessary to inquire how far this hypothesis accords with our knowledge of cell mechanics. It is only put forth as an example of the difficulties in which we land if we seek for a new function for turgor. We are, indeed, surrounded by difficulties; for, though the theories which are classed together as protoplasmic have much in their favour, they, too, lead us into an *impasse*.

Circumnutation.

I shall conclude by saying a few words about the theory of growth-curvatures put forward in the "Power of Movement in Plants." I can here do no more than discuss the relation of circumnutation to curvature, which is the thesis of the book in question, without attempting to enter the arena with regard to the many objections which have been raised to other parts of our work.

A distinguished botanist, Prof. Wiesner, of Vienna, published in 1881 a book, "Das Bewegungsvermögen der Pflanzen," entirely devoted to a criticism of the "Power of Movement" (p. 8). It is founded on a long series of experiments, and is written throughout in a spirit of fairness and candour which gives it value, apart from its scientific excellence, as a model of scientific criticism. The words written on the title-page of the copy presented to my father are characteristic of the tone of the book: "In getreuer Opposition, aber in unwandelbarer Verehrung." A letter printed among my father's correspondence shows how warmly he appreciated his opponent's attack both as to matter and manner. Wiesner's opposition is far-reaching, and includes the chief theoretical conclusion of the book—namely, that movements such as heliotropism and geotropism are modifications of circumnutation. Neither will he allow that this revolving nutation is the widely-spread phenomenon we held it to be. According to Wiesner, many parts of plants which do not circumnutate are capable of curving geotropically, &c.; he is, therefore, perfectly justified, from his own point of view, in refusing to believe that such curvatures are derivations from circumnutation. He points out that our method of observing circumnutation is inaccurate, inasmuch as the movement is recorded in oblique projection. This we were aware of, and I cannot but think that Wiesner has unintentionally exaggerated its inaccuracy; and that, if used with reasonable discretion, it cannot lead to anything like such faulty records as in the supposititious cases given by our critic. However this may be, Wiesner's results are perhaps more trustworthy than ours, and should receive the most careful consideration.

Wiesner's conclusions, taken from his own summaries, are as follows:—

The movement described as circumnutation is not a wide-spread phenomenon in plants. Stems, leaves, and acellular fungi are to be found which grow in a perfectly straight line. Some roots grow for considerable periods of time without deviating from the vertical. When circumnutation does occur, it cannot be considered to have the significance given to it in the "Power of Movement." The movements observed by Wiesner are explained by him in three different ways:—

i. As the expression of a certain irregularity in growth depending on the want of absolute symmetry in structure, and on the fact that the component cells of the organ have not absolutely similar powers of growth.

ii. As the expression of opposing growth-tendencies. Thus certain organs have inherent tendencies to curve in definite planes—for instance, the bending of the hypocotyl in the plane of the cotyledons. Wiesner believes that such tendencies, when combined with others—heliotropic, geotropic, &c.—lead to

[1] Sachs's *Arbeiten*, 1878, and "Physiology," 1886. See also Gardiner, on protoplasmic contractility, in the *Annals of Botany*, i p. 366. Pfeffer has, I think, shown that Vines's and Gardiner's theories assume the existence of too great strength in the ectoplasm. See Pfeffer in *Abhandl. der k. Sächs. Gesellsch.* xvi. 1890, p. 329.

alternate bendings in opposite directions, according as one or other of the components is temporarily the stronger.

iii. Wiesner allows that circumnutation does exist in some cases. This last class he considers a small one; he states, indeed, that "nearly all, especially the clearly perceptible circumnutations," are combined movements belonging to the second of the above categories.

Although I have perhaps no right to such an opinion without repeating Wiesner's work, yet I must confess that I cannot give up the belief that circumnutation is a widely-spread phenomenon, even though it may not be so general as we supposed.

If, then, circumnutation is of any importance, we are forced to ask what is its relation to growth-curvatures. It was considered by my father to be "the basis or groundwork for the acquirement, according to the requirements of the plant, of the most diversified movements" ("Power of Movement," p. 3). He also wrote (*loc. cit.*, p. 4):—"A considerable difficulty in the way of evolution is in part removed, for it might be asked how did all these diversified movements . . . . first arise? As the case stands, we know that there is always movement in progress, and its amplitude, direction, or both, have only to be modified for the good of the plant in relation to internal or external stimuli."

Those who have no belief in the importance of circumnutation, and who hold that movements may have arisen without any such basis, may doubtless be justified in their position. I quite agree that movement *might* be developed without circumnutation having anything to do with the matter. But in seeking the origin of growth-curvatures it is surely rational to look for a widely-spread movement existing in varying degrees. This, as I believe, we have in circumnutation: and here comes in what seems to me to be characteristic of the evolution of a quality such as movement. In the evolution of structure, each individual represents merely a single one of the units on which selection acts. But an individual which executes a number of movements (which may be purposeless) supplies in itself the material out of which various adapted movements may arise. I do not wish to imply that tentative movements are of the same order of importance as variations, but they are undoubtedly of importance as indication of variability.

The problem may be taken back a stage further; we may ask why circumnutation should exist. In the "Power of Movement" (p. 546) we wrote:—"Why every part of a plant whilst it is growing, and in some cases after growth has ceased, should have its cells rendered more turgescent and its cell-walls more extensile first on one side then on another . . . is not known. It would appear as if the changes in the cells required periods of rest." Such periods of comparative rest are fairly harmonious with any theory of growth; it is quite conceivable by intussusceptionists and appositionists alike that the two stages of elongation and fixation should go on alternately,[1] but this would not necessarily lead to circumnutation. It might simply result in a confused struggle of cells, in some of which extension, in others elongation, was in the ascendant; but such a plan would be an awkward arrangement, since each cell would hinder or be hindered by its neighbour. Perfection of growth could only be attained when groups of contiguous cells agreed to work together in gangs—that is, to pass through similar stages of growth synchronously. Then, if the different gangs were in harmony, each cell would have fair play, elongation would proceed equally all round, and the result would be circumnutation.[2] Whether or no any such origin of circumnutation as is here sketched may be conceived, there can be no doubt that it had its origin in the laws of growth apart from its possible utilization as a basis for growth-curvature.

It is, however, possible to look at it from a somewhat different point of view—namely, in connection with what Vöchting has called *rectipetality* ("Die Bewegung der Blüthen und Früchte," 1882). He made out the fact that when an organ has been allowed to curve geotropically, heliotropically, &c., and is then removed from further stimulation by being placed on the klinostat, it becomes straight again. This fact suggested to Vöchting his conception of rectipetality, a regulating power leading to growth in a straight line. It may be objected that such a power is nothing more than the heredity, which moulds the embryo into the likeness of its parent, and by a similar power insists that the shoot or root shall take on the straight form necessary to its specific character. But the two cases are not identical. The essence of rectipetality is the power of recovering from disturbance caused by external circumstances. When an organ has been growing more quickly on one side than another, the regulating power reverses this state of things and brings the curving organ back towards the starting-point. We have no means of knowing how this regulating power acts in undisturbed growth. It is possible to imagine a type of irritability which would insure growth being absolutely straight, but it is far more easy to conceive growth as normally made up of slight departures from a straight line, constantly corrected. In drawing a line with a pencil, or in walking towards a given point, we execute an approximately straight line by a series of corrections. If we may judge in such a manner by our own experience, it is far more conceivable that the plant should perceive the fact that it is not growing absolutely straight, and correct itself, than that it should have a mysterious power of growing as if its free end were guided by an external force along a straight-edge. The essence of the matter is this: we know from experiments that a power exists of correcting excessive unilateral growth artificially produced; is it not probable that normal growth is similarly kept in an approximately straight line by a series of aberrations and corrections? If this is so, circumnutation and rectipetality would be different aspects of the same thing.

This would have one interesting corollary: if we fix our attention on the regulating power instead of on the visible departures from the straight line, it is clear that we can imagine an irritability to internal growth-changes existing in varying intensities. With great irritability very small departures from the straight line would be corrected. With a lower irritability the aberrations would be greater before they are corrected. In one case the visible movement of circumnutation would be very small, in the other case large, but the two processes would be the same. The small irregular lateral curvatures which Wiesner allows to exist would therefore be practically of the same value as regular circumnutation, which he considers comparatively rare.

The relation between rectipetality and circumnutation may be exemplified by an illustration which I have sometimes made use of in lecturing on this point. A skilful bicycle-rider runs very straight, the deviations from the desired course are comparatively small; whereas a beginner "wobbles" or deviates much. But the deviations are of the same nature; both are symptoms of the regulating power of the rider.

We may carry the analogy one step further: just as growth-curvature is the continuance or exaggeration of a nutation in a definite direction, so when the rider curves in his course he does so by wilful exaggeration of a "wobble."

It may be said that circumnutation is here reduced to the rank of an accidental deviation from the right line. But this does not seem necessarily the case. A bicycle cannot be ridden at all unless it can "wobble," as every rider knows who has allowed his wheel to run into a frozen rut. In the same way it is possible that some degree of circumnutation is correlated with growth in the manner suggested above, owing to the need of regular pauses in growth. Rectipetality would thus be a power by which irregularities, inherent in growth, are reduced to order and made subservient to rectilinear growth. Circumnutation would be the outward and visible sign of the process.

I feel that some apology is due from me to my hearers for the introduction of so much speculative matter. It may, however, have one good result, for it shows how difficult is the problem of growth-curvature, and how much room there still is for work in this field of research.

NOTES.

THE German Leopold-Caroline Academy at Halle has conferred the degree of Doctor of Philosophy on the Director of the Royal Gardens, Kew.

MESSRS. MACMILLAN AND CO. hope to publish before Christmas a series of popular sketches in the history of astronomy from the earliest times to the present day, in the form of a

[1] Strasburger, "Histolog Beiträge," p. 195, speaks of the pause that must occur after the formation of a cellulose lamella. Hofmeister, *Württemburg. Jahreshefte*, 1874, describes the growth in length of Spirogyra as made up of short intervals of rapid growth alternating with long pauses of slow growth.

[2] I purposely omit the circumnutation of pulvini.

volume containing three courses of lectures on astronomical biography by Prof. Oliver Lodge, F.R.S. The work will be fully illustrated, and will bear the title "Pioneers of Science."

AT the monthly meeting of the Field Naturalists' Club of Victoria, held on July 13 last, as we learn from the Melbourne *Argus* of July 14, Messrs. Luehman and French read a note and exhibited the skin of a tree-climbing kangaroo from Northern Queensland, new to science, to which they gave the name of *Dendrolagus muelleri*. This remarkable marsupial has a body about two feet in length, with a tail somewhat exceeding two feet. The disproportion between the fore legs and the hind legs is not nearly so great as that of the ordinary kangaroo and wallaby; the toes are strong and curved, to enable it to climb tall and straight trees, on the leaves of which it exists. This tree-kangaroo is more nearly allied to the species which was discovered a few years ago in Queensland than to the two species from New Guinea. The specimen described was got from a straight tree, about ninety feet above the ground.

IN his letter on "Dredging Products" (NATURE, August 13, p. 344), Mr. Alex. Meek, writing from Shetland, gave a short *résumé* of localities where Actinotrocha has been found. As the south coast of England was not mentioned, Mr. W. L. Calderwood writes to call attention to a paper by his predecessor at the M.B.A. Laboratory, Plymouth, Mr. G. C. Bourne, published in the Journal of the Marine Biological Association, vol. i., No. 1. After mentioning the occurrence of Tornaria, Mr. Bourne goes on to say:—"Actinotrocha, the larva of Phoronis, is common. . . . Several specimens of larval Amphioxus were taken in the tow-net towards the end of October." In vol. ii. No. 1, Mr. Garstang also has a note on the occurrence of the adult Phoronis. Actinotrocha has again appeared several times during the present summer.

M. IMFELD, the Swiss engineer, who has been engaged to examine the nature of the summit of Mont Blanc for the construction there of M. Janssen's proposed Observatory, recounts in a Zürich journal the difficulties he is experiencing in his preliminary survey. M. Imfeld is staying with eight workmen and two doctors at M. Vallot's Observatory, which has an altitude of 4400 metres, and thence they proceed daily to the summit, where they work for several hours a day in the endeavour to ascertain the depth of the snow for the purpose of getting the necessary foundation for the building. M. Eiffel has expressed the opinion that the construction of an Observatory will only be possible if the snow does not exceed a depth of 12 metres. M. Imfeld states that they have encountered traces of a ridge of rock 18 to 20 metres below the summit, and covered with about 1 metre of snow. They have therefore commenced to make a series of lateral tunnels on three sides, at a distance equal to 12 metres below the summit, to ascertain if the ridge extends to that height. Progress is necessarily slow. Most of the men are suffering from *mal de montagne*. Some, however, who are engaged at M. Vallot's cabin are able to work almost as long as in the valley, and they also eat and sleep well. In spite of two coke stoves, the thermometer of the cabin never rises above zero; even ink freezes, and water boils at 83°, and they cannot properly cook meat. For a day or two they were disturbed by violent storms.

MARTINIQUE has been visited by a terrible cyclone, the most violent that has been known in the island since 1817. It lasted four hours, and was followed by an earthquake; and many lives were lost. According to the latest information received in Paris from Martinique on Monday last, the number of persons known to have perished was 340; but that did not include the sailors lost in numerous shipwrecks along the coast and at sea. Besides the persons killed, very many were injured by the falling buildings, trees, and stones. All along the coast houses were completely demolished. The town of Morne Rouge is said to be a total wreck, and Fort de France is almost entirely destroyed. Much suffering prevails among the population.

MESSRS. L. REEVE AND CO. have in preparation a new work on the British Fungi, Phycomycetes, and Ustilagineæ, by George Massee, Lecturer on Botany for the London Society for the Extension of University Teaching; a work on the British Hemiptera Heteroptera, by Edward Saunders; a new work on the Lepidoptera of the British Islands, by Charles G. Barrett; and a new work on the physiology of the Invertebrata, by Dr. A. B. Griffiths.

MESSRS. WHITTAKER AND CO. are about to publish "A First Book of Electricity and Magnetism," by W. Perren Maycock. The work is intended for the use of elementary science and art and engineering students, and general readers.

MESSRS. CASSELL AND CO. are issuing, in monthly parts, a new and revised edition of Sir R. Stawell Ball's well-known "Story of the Heavens." The first part has just been published.

THE additions to the Zoological Society's Gardens during the past week include a Common Fox (*Canis vulpes*), British, presented by Captain H. S. Tunnard; five White-eared Conures (*Conurus leucotis*) from Brazil, presented by Mrs. Arthur Smithers; four Leopard Tortoises (*Testudo pardalis*), three Angulated Tortoises (*Chersina angulata*), a Galeated Pentonyx (*Pelomedusa galeata*), a Hoary Snake (*Coronella cana*), a Robben Island Snake (*Coronella phocarum*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; two Alligators (*Alligator mississippiensis*) from Carolina, presented by Mr. Charles Downs; a Gold Pheasant (*Thaumalea picta* ♀) from China, presented by Mr. R. Hudson; a Pig-tailed Monkey (*Macacus nemestrinus* ♂) from Java, two Water Vipers (*Cenchris piscivora*) from North America, deposited.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 17.—M. Duchartre in the chair.—On a new blow-pipe, by M. Paquelin.—On "cyclic systems," by M. A. Ribaucour.—New researches on the solar atmosphere, by M. H. Deslandres. (See Our Astronomical Column.)—On the enormous velocity of a solar prominence observed on June 17, 1891, by M. Jules Fényi. M. Trouvelot has previously recorded a remarkable luminous outburst that occurred on the sun on June 17. The position-angle of the group of prominences observed by M. Fényi was about 282°. At one time the velocity of one portion of the group reached the high value of about 850 kilometres per second. And another portion was elevated through about 72′ 2″ in 210 seconds—the mean velocity being at least 485 kilometres per second. It is therefore concluded from the observations that matter can be projected from the sun into space with a velocity sufficient to prevent its falling back again.—Mechanical determination of the series of atoms of carbon in organic compounds, by M. G. Hinrichs.—On the arterial system of Isopods, by M. A. Schneider.—On the growth of the shell of *Helix aspersa*, by M. Moynier de Villepoix.

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